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Medicare Per Episode Prospective Payment: A System with No Apparent Shifts in Cost or Burden

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Ba... ...ps
Vet... ...irs San Diego Health Care System
Univers...y of California, San Diego

Programmers:

Elizabeth Stuart
Mathematica Policy Research, Inc.

Amy Zambrowski
Mathematica Policy Research, Inc.

Survey Director:

Rhoda Cohen
Mathematica Policy Research, Inc.

Submitted to:

Health Care Financing Administration
7500 Security Blvd, (C-3-21-06)
Baltimore, MD 21244-1850

Project Officer:
Ann Meadow

Submitted by:

Mathematica Policy Research, Inc.
P.O. Box 2393
Princeton, NJ 08543-2393
(609) 799-3535

Project Director:
Valerie Cheh

Principal Investigators:
Valerie Cheh
Randall Brown
Barbara Phillips

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EXECUTIVE SUMMARY

As part of its ongoing effort to study methods of providing more cost-effective care, the Health Care Financing Administration (HCFA) implemented the Per-Episode Home Health Prospective Payment Demonstration. Under the demonstration, participating home health agencies were paid a fixed, lump-sum payment for the first 120 days of each episode of care provided to Medicare beneficiaries and a predetermined rate for each visit thereafter. By allowing agencies to retain most of any surplus payments over cost, this payment method gives agencies an incentive to provide home health care in a cost-efficient manner.

Other demonstration results show that prospective payment led home health agencies to substantially reduce the number of Medicare home health visits per episode and to reduce the length of home health episodes. Such reductions in Medicare home health use could lead to increased use of *non-Medicare* services, possibly increasing demand for Medicaid and other publicly provided services and increasing time and financial burdens on patients' families. This study examined that issue using survey data on informal care provided by family members and friends, formal home- and community-based services (such as home-delivered meals and aide, homemaker and maid services), and formal residential services (assisted-living facilities and nursing homes).

For the Medicare home health population as a whole, we find that prospective payment did *not* increase the use of non-Medicare services during the eight months after home health admission. It did not increase informal care from family members or friends; formal home- and community-based services, or the use of two types of formal residential services (assisted-living facilities and nursing homes). Moreover, prospective payment appears to have reduced both the likelihood of residing in an assisted-living facility and the number of days in a nursing home (including days of Medicare-covered nursing home care).

Our results suggest that prospective payment *may* have differential effects on Medicare home health patients who are dually eligible for Medicaid and Medicare and on those who are not. Prospective payment may have increased the proportion of Medicaid patients receiving informal care.

BACKGROUND

Ninety-one agencies in five states entered the demonstration at the start of their 1996 fiscal years. Before the three-year demonstration began, the participating agencies were randomly assigned to either the treatment group or the control group. Agencies assigned to the treatment group were reimbursed under the demonstration's prospective payment method, whereas those assigned to the control group continued to be reimbursed under Medicare's method of cost-based reimbursement (the payment method for all home health agencies at the time the demonstration began).

For each patient admitted to home health care, treatment group agencies received a lump-sum payment for the first 120 days of home health care, regardless of the number of visits provided or their cost. The amount of the per-episode payment was based on each agency's own costs during the fiscal year immediately preceding its entry into the demonstration, adjusted for changes in its case mix and for inflation. An agency received a new per-episode payment for a given Medicare beneficiary only after the 120-day "at-risk" period had ended and a 45-day gap in services had occurred. For each visit after the at-risk period that did not begin a new episode (referred to as the "outlier" period), treatment agencies received a fixed payment that varied by the type of visit and that also was based on their own predemonstration costs.

RESEARCH QUESTIONS

We expected a reduction in the use of Medicare home health services under prospective payment would produce an impact on the use of non-Medicare services. During the eight-month period covered by our data, prospectively paid agencies did substantially reduce the durations of episodes and the number of visits rendered. For example, the average number of visits during the first 120 days of an episode fell from about 45 visits for cost-reimbursed agencies to 37 visits for prospectively paid agencies (Trenholm 1998). Here we address whether these changes affected the use of non-Medicare services, specifically: (1) informal care from family members and friends; (2) formal home- and community-based services, such as home-delivered meals and aide, homemaker, and maid services; and (3) two formal residential services, assisted living facilities and nursing homes. These services could be purchased privately or could be provided through volunteer programs or public programs other than Medicare, including Medicaid.

The expected *direction* of any impact on the use of non-Medicare services depended on the strategies that prospectively paid agencies adopted to reduce the number of home health visits. If they had cut back on necessary services while reducing home health visits, then patients might have *increased* their reliance on informal caregivers, on formal non-Medicare services, or on both. Conversely, the use of non-Medicare services could have *decreased* under per-episode payment if prospectively paid agencies had reduced the number of home health visits through more rapid rehabilitation of patients. Patients who are rehabilitated more quickly may require fewer non-Medicare services by the time of the interview (four and eight months after admission). Administrators of prospectively paid agencies reported that, in their opinion, the use of non-Medicare services both increased reliance on family members and home- and community-based services *and* promoted more rapid rehabilitation of patients (Phillips and Thompson 1997).

Because the expected direction of any impacts were uncertain, we formally tested hypotheses of no difference between the treatment and control groups (using two-tailed tests). Increases in the use of informal care and shifting of costs to other programs are of particular policy interest, however, as they may offset savings generated by reductions in Medicare home health services.

DATA AND METHODOLOGY

We examined data on the use of non-Medicare services collected during interviews conducted with a random sample of patients admitted to demonstration home health agencies during a six- to eight-month period in the agencies' second (or early third) years of demonstration operations. Patients were eligible for inclusion in the survey sample if their admission qualified for per-episode payment (treatment group) or would have qualified under demonstration rules (control group).

We administered two waves of telephone interviews to 2,980 sampled patients at fixed intervals after their home health admission and obtained completion rates of more than 90 percent for each wave. The first wave, referred to in this report as the four-month interview, was administered just before the end of the 120-day period covered by the lump-sum payment (that is, approximately four months after home health admission). The second wave was administered eight months after home health admission. Administration at fixed intervals ensured that we collected data at comparable points in spells of illness for the treatment and control groups, even though the length of home health episodes was reduced for patients in the treatment group.

We conducted our main analysis on patients for whom the sampled admission was the first admission during the demonstration (about 80 percent of the total sample). 2,072 patients completed the four-month interviews and 1,883 completed the eight-month interviews. The patient survey samples are large enough to detect moderate to large impacts (if such impacts exist). For example, differences of about five percentage points can be detected at the .10 level with 80 percent power. Smaller differences are not likely to be policy relevant.

The patient interviews are the source of all outcome measures on the use of non-Medicare services. Many of these are binary measures of the receipt of a given service (that is, whether the sampled patient received the service between home health admission and the four-month interview or between the four- and eight-month interviews). Measures of the *amount* of services received are available for days in a nursing home, number of visits by informal caregivers, and number of visits by aides, homemakers, and maids.

We collected measures of preadmission use of non-Medicare services during the four-month interview, and measures of the preadmission characteristics of sampled patients were obtained from data collected at admission for case-mix adjustment and from prior Medicare claims and enrollment records. We obtained data on agency characteristics from agency cost reports and from the demonstration implementation contractor.

We used logit models (for binary outcome measures) and Tobit models (for amount measures) to estimate the effect of prospective payment, controlling for preexisting differences between the treatment and control agencies in patient and agency characteristics. This approach proved crucial to obtaining valid impact estimates; despite the randomization of participating agencies, there were several significant differences between treatment and control agencies aside from the method of payment.

Because the payment intervention is implemented at the agency level, observations were weighted for the main analysis so that each agency is represented equally. To avoid overstating the

precision of the impact estimates, we adjusted the standard errors of the impact estimates to account for the effects of weighting and of clustering of patient observations within agencies. Analyses of the robustness of our estimates showed that some estimates were sensitive to weighting scheme, leading us to interpret some of our results with caution.

FINDINGS

No Impact on Informal Care Overall, but Possible Increase for Medicaid Recipients

Per-episode payment had no material, overall impact on the likelihood of receiving informal care during the eight months after home health admission. Although our measures are limited, we also find no impact on the amount of informal care received.

However, the overall finding of no impact on informal caregiving may mask an impact for beneficiaries who are dually eligible for Medicare and Medicaid. The likelihood of receiving informal care four to eight months after home health admission may be greater for Medicaid recipients who are patients of prospectively paid agencies than for those who are patients of cost-reimbursed agencies. We temper this conclusion with caution because we find no impacts for the first few months after home health admission, and we have no plausible explanation for such a pattern of impacts. Furthermore, some of the results for months four through eight are sensitive to weighting scheme.

No Impact on Home- and Community-Based Services

Per-episode payment did not have an overall impact on the likelihood of receiving formal home- and community-based services during the eight months after home health admission. This payment method also had no overall impact on the amount of these services that patients received.

Assisted-Living Facility Residence and Nursing Home Use Did Not Increase and May Be Reduced

Per-episode payment certainly did not increase, and appears to have reduced, the use of residential services. The likelihood of residing in an assisted-living facility apparently fell by about a third during the first four months after home health admission and may have been reduced for the entire eight-month period covered by the interview data. Prospective payment also appears to have reduced the average number of days of nursing home care by about two-thirds several months after home health admission. This pattern suggests that nursing home admission was delayed by a few weeks or months on average; however, we have only weak evidence of a reduction in the likelihood of nursing home admission during the first four months after home health admission.

We have no supporting evidence about the mechanisms underlying these apparent reductions in the use of residential services. We hypothesized that reductions in the use of medical social work by prospectively paid agencies might lead to reductions in the use of residential services because

medical social workers would be less often available to facilitate residential placement. However, we find no evidence to support that hypothesis. Patients and families might have taken more personal responsibility for care in response to the urging of staff of prospectively paid agencies (relative to patients of cost-reimbursed agencies and their families), and thereby reduced use of assisted-living facilities, but we have no direct evidence of that.

We observed the apparent reductions in the use of these two types of residential facilities for patients who are dually eligible for Medicaid and Medicare. We also observed these reductions for patients who are eligible only for Medicare.

Findings Were Robust Across Different Types of Agencies

We find no evidence that impacts on the use of non-Medicare services vary for for-profit and nonprofit agencies or for agencies with high-use and low-use practice patterns in the predemonstration period.

LIMITATIONS OF THIS ANALYSIS

This study has important limitations. However, none of them cast doubt on the central finding that prospective payment did not cause sizeable increases overall in the use of non-Medicare services.

Demonstration Agencies May Have Behaved Differently from Agencies Nationally

A number of possible differences between agencies participating in the demonstration and agencies that did not participate may lead our findings to overstate or understate the nationwide impacts of prospective payment. As in any study design based on voluntary participation, the agencies in this demonstration may reflect the group most able (or willing) to respond to the incentives of the intervention, so that demonstration impacts may overstate the actual reductions in home health visits that would take place nationally under a similar payment system. In this case, use of non-Medicare services would be less likely to increase than in the demonstration. In addition, if the practice patterns of agencies nationwide systematically differ from those of demonstration agencies, we would probably misstate the effects of a nationwide program on home health visits, which could drive differences in impacts on the use of non-Medicare services.

A National Payment System Would Differ from a Demonstration System

Any national program of prospective payment would almost certainly differ from the one implemented for the demonstration. For example, agencies would not be protected from incurring financial losses under a national system, which might lead some agencies to respond more cautiously to program incentives. Moreover, an agency's payment would almost certainly not be based on its own prior cost per episode; instead, it would likely be based on a national average. For many

agencies, such as those with high-use practice patterns, this difference in payment might actually lead to larger cuts in home health visits than were observed under the demonstration. Again, different impacts on home health visits could lead to different impacts on the use of non-Medicare services.

Statistical Power Is Not Adequate to Detect Small Impacts

We cannot rule out the possibility that per-episode payment has small, overall impacts on the use of non-Medicare services, which we do not have sufficient statistical power to detect. However, the power is sufficient to detect overall impacts sufficiently large to be policy relevant. For patient and agency subgroups, only relatively large impacts can be detected with confidence.

Findings Are Based on Admissions During a Limited Period

To provide time for prospectively paid agencies to develop and implement strategies in response to prospective payment, we did not begin selecting the patient survey sample until the second year of demonstration agencies' operations. Nonetheless, if agencies continued to develop strategies to reduce home health visits beyond their second demonstration year, the reductions might at some point result in increased non-Medicare services. It is therefore possible, that our findings (and indeed any findings based on two years of experience with an intervention) may not accurately reflect the long-term effects of prospective payment.

POLICY IMPLICATIONS

The findings of this report provide important information on the effects of prospective payment at a time when HCFA, under mandate from the 1997 Balanced Budget Act, must design and implement a nationwide prospective payment system.

Prospective Payment for Medicare Home Health Care Would Not Increase Reliance on Informal Caregivers or Other Non-Medicare Services

We find that per-episode prospective payment does *not* increase the overall use of informal care, formal home- and community-based services, or formal residential services--despite sizeable reductions in the provision of Medicare home health services. These reductions in Medicare home health may produce substantial savings for the Medicare program and thus to society as a whole. The findings of this analysis indicate that *societal* savings will not be offset by increased costs for services purchased by private individuals and/or provided by volunteer and public programs, including the Medicaid program, nor by increased reliance on family members and friends of Medicare home health patients.

I. THE PER-EPISODE HOME HEALTH DEMONSTRATION AND EVALUATION

The Health Care Financing Administration's (HCFA's) Per-Episode Home Health Prospective Payment Demonstration tested the extent to which prospective payment for Medicare home health services increases the efficiency of service provision. This efficiency is meant to reduce public expenditures while maintaining access to and quality of care. Per-episode payment encouraged efficiency by giving agencies the financial incentive to reduce their costs. Specifically, under the demonstration payment system, any savings that produce lower costs per episode of patient care could generate profit for an agency.[1] These incentives differ greatly from those under the current system of cost-based reimbursement, which provides no reward for cost containment.

In this report, we present findings from an analysis of impacts on patients' use of non-Medicare services, including informal care provided by family members and friends, during the agencies' second year of demonstration operations. A finding of impacts on non-Medicare services would indicate that home health agencies have shifted the burden of providing care to informal caregivers and have shifted costs to private and volunteer programs (such as those funded by charities or operated by some churches) or to other public programs (such as Medicaid and programs operated by Area Agencies on Aging).

This report considers short- and longer-run impacts on the use of non-Medicare services (that is, impacts during the first eight months after home health admission). The data are drawn primarily

[1]Strictly speaking, only for-profit agencies earn profits; nonprofit agencies generate surpluses that cannot be distributed to "owners" of the organization. However, for brevity, we use the term "profits" in this report to refer to surpluses generated by nonprofit agencies, as well as profits earned by for-profit agencies.

from telephone interviews administered shortly before the fourth month after admission (the 120th postadmission day) and eight months after admission.

The remainder of this chapter gives an overview of the Medicare home health benefit and the Medicare-certified home health industry and describes the Per-Episode Home Health Prospective Payment Demonstration. The reader who is familiar with Medicare home health may wish to read only Section B, which describes the demonstration procedures.

A. THE MEDICARE HOME HEALTH BENEFIT

Congress established the Medicare home health care benefit in 1965, when the original Medicare program was created. Home health benefits were included to offer beneficiaries with acute conditions a less intensive and less expensive alternative to inpatient hospital care. The home health benefit has been modified at various times since the inception of the Medicare program, partly to increase access to care.

The current Medicare home health benefit covers home health services primarily under Part A, and deductibles and coinsurance do not apply.[2] To be eligible for home health benefits, a beneficiary must (1) have Medicare coverage; (2) be homebound; (3) be under the care of a physician; and (4) need skilled nursing, physical therapy, or speech therapy on a part-time or intermittent basis.[3]

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[2]In some rare instances, such as when an individual does not qualify as homebound or does not have Part A coverage, home health care may be covered under Medicare Part B. In cases of Part B home health, deductibles and copayments do apply. In addition, under the Balanced Budget Act of 1997, the long-term use of home health services was transferred to Part B. However, our analysis period pre-dates this change.

[3]Skilled nursing services are covered as long as (1) a physician has ordered them, (2) they are required on a part-time or intermittent basis, (3) they require the skills of a registered nurse (or of a licensed practical nurse or licensed vocational nurse under a registered nurse's supervision), and (4) they are reasonable and necessary to treat an illness or injury. Physical therapy and speech therapy are covered if a physician's assessment recommends these services. Beneficiaries who need (continued...)

Coverage under the home health benefit broadened considerably after the settlement of a lawsuit against HCFA in 1989; the broader coverage in turn, contributed to dramatic growth in Medicare home health expenditures after that time. Medicare spending for home health care increased from $2.5 billion in 1989 to $16.8 billion in 1995, more than tripling the share of total Medicare outlays spent on home health (U.S. General Accounting Office 1998). Nearly all this growth has been the result of an increase in the provision of services, which has coincided with a dramatic expansion in industry size. In 1989, there were roughly 5,700 Medicare-certified home care agencies; by 1997, this figure had risen to more than 10,000.

As the use of home health services has grown nationwide, service use patterns have continued to differ strikingly across regions. For example, agencies provided an average of 47 visits during an episode of care to beneficiaries admitted to home health in 1990 and 1991, with the mean episode lasting 94 days. However, the average number of visits per episode varied from a low of 28 in the Pacific region to a high of 95 in the East South Central region, and the respective mean episode durations ranged from 60 days to 180 days (Schore 1995). By 1996, Medicare home health recipients received an average of 74 visits nationally, and the average varied from 48 in the Middle Atlantic and Pacific regions to 110 and 133 in the East and West South Central regions, respectively (Health Care Financing Administration 1998). By 1997, the average number of visits had increased to 69 nationwide, but ranged from a low of 30 visits in Washington State to a high of 153 visits in Louisiana (U.S. General Accounting Office 1998).

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[3](...continued)
only occupational therapy are entitled to benefits only if they have established a prior need for skilled nursing care, speech therapy, or physical therapy in the current or previous certification period (see Teplitsky and Janson 1985-1992).

The dramatic growth in Medicare home health expenditures, combined with striking regional variation in service use and recent investigations of industry fraud and abuse, prompted Congress to legislate changes to the Medicare home health benefit as part of the Balanced Budget Act of 1997. The act mandates the implementation of a per-episode prospective payment system for Medicare home health by October 2000. Other changes to the home health benefit under the Balanced Budget Act include:

- Reducing the per-visit cost limit (known as the Section 223 Limits) from 112 percent of the mean to 105 percent of the median cost for freestanding agencies in the region. The Section 223 Limits were frozen for reporting periods that began between July 1, 1994, and June 30, 1996.
- Using a new algorithm to define the maximum payment for an agency. The algorithm is based on annual per-beneficiary costs or per-visit costs in a base year, whichever is lower. It is commonly referred to as the Interim Payment System (IPS), because it is intended to last only until the mandated prospective payment system takes effect.
- Eliminating coverage for blood drawing when it is the only home health service required
- Redefining "part-time" and "intermittent" care
- Redefining service location on the basis of the location of the patient rather than of the agency
- Requiring additional billing information (specifically, an identifier for admitting physician and visit duration)

The demonstration agencies are not subject to the changes in either the per-visit cost limit or the payment algorithm until after their participation in the demonstration ends. However, they are affected by the other changes.

B. THE PER-EPISODE DEMONSTRATION

Because a cost-reimbursed payment system does not provide a mechanism for home health agencies to earn profits by cutting costs, it offers no incentive for them to provide services

efficiently. Moreover, by reimbursing costs up to allowable limits, the system effectively subsidizes inefficient providers. The intent of per-episode prospective payment was to increase efficiency by using the profit motive as the primary incentive. Ninety-one Medicare-certified home health agencies in five states--California, Florida, Illinois, Massachusetts, and Texas--enrolled in the three-year per-episode demonstration.[4] Forty-seven of the agencies were randomly assigned to the treatment group and received per-episode payment. The remaining 44 were assigned to the control group and continued to operate under cost reimbursement. One control agency transferred into the treatment group near the start of the demonstration, leading to a revised total of 48 treatment agencies and 43 control agencies.[5] Each agency entered the demonstration and began implementing prospective payment at the start of its fiscal year. The first agencies in the treatment group began implementing prospective payment in June 1995; the latest entrants began in January 1996. Demonstration operations were planned to continue through December 1998. (Later, the demonstration was extended for treatment group agencies until Medicare home health prospective payment is implemented nationally.)

Mathematica Policy Research, Inc. was the evaluation contractor responsible for the process and impacts evaluations of the demonstration. Some of this analysis took place under a subcontract to the Veteran's Medical Research Foundation. Several other organizations also participated in the demonstration. Abt Associates, Inc. was the implementation contractor responsible for recruiting

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[4]Reflecting the United States more generally, the provision of Medicare home health varied considerably in the demonstration states. In 1997, the average visits provided per beneficiary were as follows: California, 47; Illinois, 47; Florida, 70; Massachusetts, 89; and Texas, 134 (U.S. General Accounting Office 1998). In a future report, we will compare the demonstration agencies and agencies nationwide on home health provision.

[5]The transfer was made at the request of the agency that had established a network with two other agencies assigned to the treatment group. The three agencies plans to merge fully at the end of the demonstration.

demonstration agencies, monitoring the status of demonstration operations, and calculating certain statistics used to compute agency payments. Palmetto Government Benefits Administrator (PGBA) was the fiscal intermediary responsible for reviewing the claims of and payments to both treatment and control agencies. The Center for Health Policy Research (CHPR) at the University of Colorado was responsible for designing and implementing a quality assurance system for the demonstration agencies.

1. Demonstration Payment and Incentives

HCFA developed the Home Health Prospective Payment Demonstration to assess whether the profit motive can increase efficiency in providing Medicare home health care--and thereby reduce public expenditures--without sacrificing either access to care or the quality of care. Phase I of the demonstration, which tested per-visit prospective rate setting, gave agencies an opportunity to generate profits (and avoid losses) by reducing per-visit costs.[6] The evaluation of that demonstration found that some agencies did earn profits, but also did little to contain overall costs because there was no incentive to limit the services they provided. Phase II tested a per-episode prospective payment system. This system encourages agencies to reduce their per-visit costs and provides an additional incentive to reduce visits per patient episode. Thus, the opportunity to achieve cost savings was much greater under the Phase II payment system than under the Phase I system.

[6]Although the per-visit demonstration was implemented in the same five states as is the per-episode demonstration, most of the agencies participating in the per-episode demonstration did not participate in the per-visit demonstration. (Only agencies in the per-visit control group were eligible to do so.) For details of the results of the per-visit demonstration, see Brown et al. (1995).

a. Payment

Agencies selected for the treatment group received a lump-sum payment for the first 120 days of home health care, regardless of the number or cost of visits provided.[7] The agencies are thus "at risk" for the costs of care incurred during this period. An agency can receive a new per-episode payment for a given Medicare beneficiary only after the 120-day at-risk period and a subsequent 45-day gap in services have elapsed. For each visit after the 120-day at-risk period that was not part of a new episode (referred to as "the outlier period"), treatment agencies received a fixed payment rate that varied by the type of visit. In the demonstration, a treatment agency was also paid on a per-visit basis for visits made to patients admitted before the agency began demonstration operations ("phase-in" visits), and for visits made to those admitted within 120 days of the end of demonstration operations in that agency ("phase-out" visits).

Payments to agencies selected for the control group were based on the cost-based system in place at the start of the demonstration. Specifically, control agencies were reimbursed for their actual per-visit costs, up to 112 percent of the mean cost incurred by all agencies (for the agency's mix of visits) in the same geographic area.

b. Prospective Rate Setting

Prospective (per-episode) rates were based on a treatment agency's per-visit costs and episode profile during the fiscal year preceding its entry into the demonstration (the base year). The episode

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[7]Durable medical equipment, nonroutine medical supplies, and Part B home health services continued to be reimbursed at cost throughout the demonstration. In addition, if an agency had not provided one or more of the six Medicare services during the base year but began to do so during the demonstration, then those visits were also reimbursed at cost during the demonstration, as were the costs of care for which Medicare is a secondary payer. The six Medicare home health services are (1) skilled nursing, (2) physical therapy, (3) occupational therapy, (4) speech therapy, (5) home health care, and (6) medical social work.

profile was determined by the average number of each of the six types of Medicare-covered visits that the agency provided during the first 120 days of the base-year episode. To obtain the per-episode payment for a demonstration year, the base-year episode profile used the agency's base-year per-visit costs adjusted for inflation. Payments for outlier, phase-in, and phase-out visits were based on the agency's base-year per-visit costs (adjusted for inflation).[8] HCFA's market basket was used to adjust both the per-visit and per-episode rates for inflation. Per-episode and per-visit payment rates were subject to HCFA's statutory home health cost limits.

The case-mix adjuster classified each patient into 1 of 18 groups on the basis of 12 variables that describe the patient's characteristics. From this information, an aggregate case-mix index was created for each agency. At the end of each year of the demonstration, an agency's case-mix index for that year was compared with its case-mix index for the base quarter (the last quarter of the base year). If the agency's case mix differed, its aggregate payment was adjusted retrospectively.

c. Profit Sharing and Loss Sharing

To counteract the incentive to dramatically reduce services at the expense of quality, as well as to prevent agencies from realizing excessive surpluses at public expense, HCFA shared in any profits above selected levels. If the total of a treatment agency's per-episode and per-visit prospective payments was greater than the costs for services covered by the payments, any profit greater than five percent of total allowable costs for these services was subject to profit sharing. HCFA's share

[8]Because complete data for episode profiles and settled cost reports were not available for a given year until some months after that year had ended, the initial lump-sum and per-visit rates used in the demonstration were preliminary. They were revised after final base-year data became available.

was one-quarter for profits ranging from 5 to 15 percent. Its share increased for profits of more than 15 percent by an amount that varied with the demonstration year.

To encourage agencies to participate in the demonstration, HCFA provided a loss-sharing arrangement. HCFA reimbursed treatment agencies for 99 percent of losses in the first demonstration year, and for 98 and 97 percent of losses in the second and third demonstration years, respectively, as long as total payments were within the demonstration cost limits.

The demonstration cost limits were calculated as the sum of two subtotals: one for episodes and one for outlier, phase-in, and phase-out visits. The episode subtotal was obtained by costing out the agency's base-year episode profile at the cost limits for each demonstration year. The visit subtotal was obtained by costing out outlier, phase-in, and phase-out visits at the cost limits in that year.

2. Other Demonstration Procedures

Three important demonstration procedures involved the methods for billing, the process of the medical review, and the requirements for QA. Treatment agencies were strongly affected by all three of these procedures; control agencies were principally affected by the QA requirements. In this section, we briefly discuss each procedure.

a. Billing

To initiate an episode of care, treatment agencies had to submit an admission bill to PGBA. Treatment agencies also had to submit interim bills for services provided during the remainder of the at-risk period, although they received payments only for supplies.[9] For services provided near the start of the outlier period, an agency had to "split" its standard bill into two bills--one for services

[9]Agencies also had to submit interim bills to receive payment during outlier periods and to correctly calculate costs for profit or loss sharing with HCFA.

provided up to the end of the at-risk period, and one for services provided from the start of the outlier period.

When a treatment-agency patient was discharged during the at-risk period or the outlier period, agencies were required to submit a discharge bill to terminate the episode. PGBA would not initiate a new episode for a patient unless a previous episode had been terminated. In addition, before initiating a new episode, PGBA determined whether the 120-day risk period and a 45-day gap had elapsed.

Periodic interim payments (PIPs), a means of "smoothing" cash flow for home health agencies, originally were discontinued for treatment agencies. However, a similar periodic payment system (termed "biweekly interim payments") was later introduced by PGBA to address cash flow problems in some agencies. These problems were primarily caused by delays in agency billing and by the requirement to review 100 percent of admission claims for medical appropriateness (see discussion below).

Control agencies continued to submit bills under cost-reimbursement and remained eligible to receive PIPs. PGBA based the amount of a PIP on an agency's average cost for each type of visit. For some agencies, this method differed from the practices of their predemonstration FIs, which had based the PIP on overall agency average cost per visit. The new method may have created minor differences in the PIPs that some control agencies received before the demonstration compared with those received after they entered the demonstration.

b. Medical Review

For agencies in the treatment group, PGBA conducted a limited medical review (known as an "abbreviated" medical review) of the care delivered during the at-risk period of the patient episode. This review attempted to determine whether the patient met the coverage criteria for home health

care and whether the agency provided at least one visit that met these criteria. As a condition of payment, in addition to the admission bill, a treatment agency had to submit either (1) HCFA 485 and 486 forms (which contained information on the patient's health and eligibility status, as well as the home health plan of treatment); or (2) clinical notes for the admission that coincided with an episode eligible for prospective payment. The medical review process was based on these materials. All services paid for under per-visit rate setting (for example, those delivered during the outlier period) were subject to the usual medical review, under which PGBA reviewed a sample of claims to ensure that each visit was medically reasonable and necessary.

Initially, all episodes were subject to abbreviated medical review; in May 1996, however, Medicare reduced the proportion to 25 percent. It made this change for two reasons: (1) the claims review process took longer than expected, and (2) 100 percent medical review was considered unrealistic under a national program.

The Medical review for control agencies continued under the nondemonstration regulations. For some control agencies, however, PGBA's review procedures may have differed from those of their predemonstration FIs, requiring them to adopt some minor procedural changes.

c. Quality Assurance

All demonstration agencies were required to collect and submit patient-specific information to CHPR, the demonstration QA contractor. The QA procedures followed a continuous quality improvement approach. Visiting staff from demonstration agencies were required to collect information (primarily on functional status and medical condition) at admission and at discharge, or 120 days after admission, whichever came first. Similar information was also collected before admission to an inpatient facility for stays of 48 hours or longer and after patients returned to home health care after these inpatient stays. CHPR used this information to develop profiles describing

patient outcomes at each agency. CHPR then gave each demonstration agency its own profile to help it improve the quality of care it provided. The first profiles were sent to agencies in August 1997, during the period covered by this analysis.

II. MECHANISMS, STRATEGIES, AND IMPACTS ON HOME HEALTH USE

As described in Chapter I, per-episode prospective payment created an incentive for home health agencies to reduce the cost of home health care. They were most likely to do so by reducing the number of home health visits they rendered.[1] However, such a reduction could have affected the use of non-Medicare services.

In this chapter, we consider the mechanisms by which a reduction in the use of home health care could have affected the use of non-Medicare services. We then briefly review the strategies agencies used to reduce the number of home health visits, key findings from the impact analysis on the reduction in the use of home health services under per-episode payment, and the expected impact of the strategies on non-Medicare service use.

A. MECHANISMS

If prospectively paid home health agencies had cut back on necessary services as they reduced the number of home health visits, the burden (and, in some cases, costs) could have shifted to others, including:

- Family members and friends, to provide care directly (informal care)
- Patients or their families, who purchase services privately or through their private insurance
- Volunteer programs, such as those funded by charities or operated by some churches

[1]In theory, changes in the cost of a home health visit might affect the use of non-Medicare services if the changes in per-visit cost were related to changes in the quality of care. Per-visit costs increased during the early months of the demonstration, but there is no evidence that these changes were related to the quality of care (Cheh and Trenholm 1998). Results on impacts on per-visit costs are not yet available for later periods.

- Public programs, such as those funded by Medicaid and Area Agencies on Aging

This shift in burden and costs could have occurred in several ways. First, to make up for the reduction in the number of visits by home health staff (especially by home health aides), prospectively paid agencies could have referred a larger proportion of their patients to non-Medicare home- and community-based services or could have done so earlier in the home health episode, thereby increasing the use of these services (relative to use by patients of cost-reimbursed agencies). Agencies could have relied on their nurses to increase routine referrals to home- and community-based services in conjunction with providing nursing care or could have relied on medical social workers. Even if *agencies* had not changed their practices on referral to non-Medicare services, patients and families might have been more likely to seek out these services independently if they had believed that their needs were not being fully met.

Second, agencies could have increased the amount of informal care by asking patients and their families to assume greater responsibility for the provision of care. The agencies might have requested that this care be provided during the home health episode, or they might have requested that patients and families take full responsibility for care more quickly as episode durations decreased.

Third, to reduce the duration of home health episodes, it is conceivable that agencies could have encouraged a larger proportion of patients to enter nursing homes or other residential facilities, or to do so earlier in their home health episodes. Medical social workers could have been involved earlier to assist patients and families in making a decision about placement in a residential facility, or to facilitate the arrangements for placement.

It is also possible that the use of non-Medicare services could have *decreased* under per-episode payment. To reduce the number of home health visits, agencies could have promoted more rapid

rehabilitation of patients (for example, by providing more aggressive therapy or care by wound care specialists). In these cases, patients would have required fewer home health visits and would have been discharged more quickly. Patients who are rehabilitated more quickly need fewer non-Medicare services.

It is also possible that prospective payment could have had differential effects on informal and formal care. For example, relative to the care given to patients of cost-reimbursed agencies, families taking on more responsibility for patient care might have increased the amount of care they provided, while using the same amount or even reducing the use of formal services.

Finally, it is conceivable that the greater reliance on non-Medicare services by some patients could have been offset wholly or partly by more rapid rehabilitation of other patients in the prospective payment system. In this case, prospective payment would not have had any *net* impact on the use of non-Medicare services.

B. AGENCY STRATEGIES

Early in the demonstration, prospectively paid agencies reported pursuing (or planning to pursue) two strategies to reduce the number of home health visits rendered: (1) increased reliance on non-Medicare services, and (2) more rapid rehabilitation of patients (Phillips and Thompson 1997). Some agencies reported relying more heavily on family members and community support groups. Some reported placing greater reliance on medical social workers to arrange for necessary community support, and to help patients needing nursing home placement make the decision to seek and arrange for placement.

Other agencies reported rehabilitating patients more rapidly as a means of reducing the number of visits per episode (Phillips and Thompson 1997). They described altering their mission from "taking care of patients" to "making the patients as well and as independent as possible, as soon as

possible." Some agencies also planned to hire more staff with specialized credentials, to speed rehabilitation.

C. IMPACT OF PER-EPISODE PAYMENT ON HOME HEALTH SERVICES

We expected an impact on the use of non-Medicare services to result primarily as patients, their families, and agency staff reacted to the reduction in home health services provided to patients of prospectively paid agencies. According to Trenholm (2000), the average number of home health visits during the first 120 days of an episode decreased by 17 percent, from the expected 45 visits (the mean for patients of cost-reimbursed agencies) to about 37 visits for patients of prospectively paid agencies. Similarly, the average number of home health visits during the fifth through eighth months after home health admission fell by about 33 percent, from about 18 visits for patients of cost-reimbursed agencies to about 12 visits.

In both periods, reductions in the number of skilled nursing and home health aide visits accounted for about 90 percent of the reduction in total visits (Trenholm 2000). The number of other services provided dropped as well.

Determining the impacts on the number of medical social work visits is particularly important for the analysis of non-Medicare services. The number of these visits decreased during months 1 through 4 and months 5 through 8 after home health admission. Moreover, the likelihood of receiving any medical social work visit during the latter period fell. Agency staffs' comments to us during our site visits led us to expect that patients of prospectively paid agencies would receive medical social work visits earlier in their home health episodes. However, the timing of these visits did not change.

The proportion of patients receiving physical or occupational therapy decreased, as did the average number of physical and occupational therapy visits. However, the decreases were statistically significant only during the fifth through eighth months after admission.

The duration of home health episodes fell under per-episode payment (Trenholm 2000). By the end of the first 120 days after home health admission, about 38 percent of patients of cost-reimbursed agencies remained in home health care, compared with 30 percent of patients of prospectively paid agencies. At 240 days (about eight months) after admission, 24 and 17 percent, respectively, received this care.

D. EXPECTED IMPACT ON THE USE OF NON-MEDICARE SERVICES

We expected per-episode payment to affect the use of non-Medicare services, but the direction of impacts was unclear a priori. The incentives that agencies faced under per-episode prospective payment, the strategies they planned to implement in response to prospective payment, and the reductions in the use of Medicare home health services they achieved suggested that non-Medicare service use would change, but that the direction of the change was unclear. Thus, we tested null hypotheses stating that per-episode payment would have no effect on the use of non-Medicare services, using two-tailed tests.

III. DATA AND VARIABLES

This analysis of the impact of per-episode prospective payment on non-Medicare services relies primarily on data collected during patient interviews from a sample of patients eligible for demonstration payment, roughly four and eight months after admission to a home health agency. In this chapter, we briefly describe the survey sample and the fielding of the interviews. Then we describe the variables on the use of non-Medicare services constructed from the survey data. We also describe the secondary data used to construct control variables for our analysis.

A. THE PATIENT SURVEY

The patient survey was administered in two waves of computer-assisted telephone interviews. Both waves of interviews of the patient survey were administered a specified number of days after home health admission, rather than at discharge or a specified number of days after discharge. This approach ensured that the data for both the treatment and control groups were collected at a comparable point in a spell of illness. Thus, differences in the timing of data collection for the treatment and control groups cannot be confounded with the effects of the payment methodology.

The first wave of the patient interview was administered four months after home health admission, and the second interview was administered eight months after admission. To capture short-run impacts of the lump-sum payment, the four-month interview was administered shortly before the end of the 120-day period covered by the lump-sum payment. The vast majority of the four-month interviews were completed by the 120th day, as planned, and there was essentially no difference in the proportion of the treatment and control groups for which the interview was completed by the 120th day. The eight-month interview was designed to capture any longer-run effects of the lump-sum payment, as well as any effect of per-visit payments during the outlier

period. Both interviews provide "snapshot" measures of the use of Medicare services shortly before the interview, as well as measures covering the entire four-month interval since home health admission or the previous interview. Appendix A presents items from the four-month and eight-month interviews used in this analysis. (The wording presented is that used when patients were responding for themselves.) The completion rate was over 90 percent for both the four- and eight-month interviews.

Sample intake for the four-month interview began with demonstration patients admitted in January 1997 and ended with patients admitted in December 1997. The intake period varied by agency, with intake beginning first for agencies enrolling earliest in the demonstration. For most agencies, intake continued for six months, with this six-month period falling during the middle or latter part of the agency's second year in the demonstration. Most home health admissions during the sample intake period were identified shortly after home health admission from PGBA claims files, although some were identified by the agencies.

The patient survey sample was designed to permit weighting observations to represent agencies equally or to represent them according to their size without introducing extremely large weights for some observations under either weighting scheme, and thereby seriously reducing the precision of our estimates. We selected a set fraction of demonstration admissions to each agency, provided that fraction was expected to yield at least a minimum of 15 completed eight-month interviews per agency and no more than a maximum of 100. Our targets were 2,500 completed four-month interviews and 2,000 completed eight-month interviews.

For both the four- and eight-month interviews, we created "all-admissions" and "first-admissions" samples. The all-admission sample for the four-month interview consists of 2,525 cases for which an interview was completed and the case was not lost to analysis (primarily due to

ineligibility for demonstration payment). The first-admission sample for the four-month interview consists of 2,072 observations on patients who met the same criteria as the "all admission" sample, and for whom the sampled admission is the first demonstration home health admission. The all-admission sample for the eight-month interview consists of 2,291 cases, while the first-admission sample consists of 1,883 cases. All of these samples are split about equally between the treatment and control groups.

After the analysis was completed we identified 112 cases that were ineligible for demonstration payment that had not been excluded due to oversight. These were primarily cases in which Medicare was the secondary payer and cases in which the patient was a member of a particular type of managed care organization for which claims appear on Medicare files. We excluded these cases and re-estimated our impact coefficients for key dependent variables. There were no material differences in our estimates.

Appendix B provides a detailed description of the design of the patient survey, the patient survey sample and the fielding of the survey.

B. VARIABLES

In this section, we describe the outcome and control variables used in the analysis. All the outcome variables were constructed from the patient survey data. Although a few of the control variables were also constructed from survey data, most were constructed from secondary data.

1. Outcome Variables

The outcome variables on the use of non-Medicare services describe:

- Formal residential services, including assisted-living facilities and nursing homes

- Formal home- and community-based services, including adult day care; home-delivered meals; non-Medicare nursing visits; and non-Medicare aide, homemaker, or maid visits
- Informal care by family members and friends

a. Residential Services

We use the term "assisted-living facility services" to refer to residential services other than nursing home services, including personal care homes, supported living, board and care homes, and units of continuing care retirement communities. Because different terms are used to refer to these services, we posed the question in terms of "living in a place with staff to help with personal care, such as eating or bathing, or with housework, laundry, meal preparation, or transportation."

We created measures of whether a patient lived in an assisted-living facility at any time between home health admission and the four-month interview and between the four- and eight-month interviews. That is, these measures covered the entire period of interest, rather than simply a snapshot period.

Our measures of nursing home services included *all* nursing home services. In posing our questions, we did not attempt to rule out nursing home services covered by Medicare because our experience suggested that respondents would not have been able to accurately differentiate Medicaid and Medicare as payment sources. To overcome this measurement limitation, we devised a strategy to draw on secondary data for analysis of Medicare-covered nursing home services, and to draw on these survey data for analysis of all nursing home services.[1]

We obtained from each interview a binary measure of nursing home admission covering the four-month period of interest and a measure of the number of days in a nursing home, which covered

[1]See Schore (2000) for a discussion of the use of secondary data to analyze nursing home services covered by Medicare.

the same period. Because patients living in nursing homes were not eligible for demonstration home health services, sampled patients could receive nursing home services between home health admission and the four-month interview only if they were admitted to a nursing home during that period. To facilitate comparison of the results for the four- and eight-month data, we also measured nursing home admissions between the four- and eight-month interviews.

Although the interviews ascertained the *number* of nursing home admissions, we did not pursue analysis of this information because too few patients had more than one nursing home admission. For example, only 11 of the 116 patients admitted to a nursing home before the four-month interview were admitted more than once during that period.

Both the four- and eight-month interviews included questions on payment for nursing home care. These questions ascertained (1) whether private insurance covered any of the costs, (2) whether the patient incurred any out-or-pocket costs for nursing home services, and (3) what amount of out-of-pocket costs was incurred. Many respondents did not know the information needed to respond or did not understand these questions. For example, roughly one-fourth of the respondents reporting in the four-month interview that they had had a nursing home admission also reported that they or their families incurred some out-of-pocket costs; about half reported that they did not know whether they had out-of-pocket costs. Our concerns about the accuracy of the data convinced us not to pursue analyses of these questions on the source of payment for nursing home care.

b. Home-Based and Community-Based Services

We analyzed several measures of whether patients received formal home- and community-based services between home health admission and the four-month interview and between the four- and eight-month interviews. The basic measures indicated whether the sample member received each of four services: (1) adult day care; (2) home-delivered meals; (3) non-Medicare nursing services;

and (4) non-Medicare aide, homemaker, and maid services. We also constructed a measure indicating whether the patients received any of the four home- and community-based services during the period. Finally, we collected snapshot measures of the number of visits received from non-Medicare aides, homemakers, and maids during the two weeks before each interview.

Because of concerns about data quality, we did not pursue analysis of questions on the volume of nursing services and the receipt of privately paid services. Both the four- and eight-month interviews included a question on the number of non-Medicare nursing visits received during the two weeks before the interview. [2] However, relative to the number of cases reporting receipt of these visits, a substantial number of cases had missing values for these questions, suggesting that even respondents who did answer the question may not have been able to respond accurately. As a result of similar concerns about data quality, we decided not to analyze questions on the receipt of privately paid nursing services and privately paid aide, homemaker, and maid services.

c. Informal Care

We designed somewhat different measures of the use of informal care for the four-month and the eight-month interviews. The eight-month interview was designed to focus on longer-run impacts on informal care; therefore, it emphasized snapshot measures pertaining to the two weeks preceding that interview. In both interviews, we collected measures covering the four-month period of interest (that is, between home health admission and the four-month interview or between the two interviews). The available measures of informal care and the interview from which they are drawn are:

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[2]If a respondent had difficulty distinguishing between Medicare and non-Medicare nursing services, we mentioned the sampled demonstration home health agency by name and asked about nurses from organizations *other than* that agency.

- Measures covering the preceding four-month period

  - Whether received any informal care (four-month interview, eight-month interview)
  - Whether received informal care from a live-in caregiver (four-month)
  - Whether received informal care from a visiting caregiver (four-month)

- Snapshot measures (covering the two week preceding the interview)

  - Whether received informal care from a visiting caregiver (four-month, eight-month)
  - Number of visits received from visiting caregiver (four-month)[3]
  - Whether received informal care for specific tasks of activities of daily living (ADL), such as eating (eight-month)
  - Whether received informal care for specific tasks of instrumental activities of daily living, such as meal preparation (eight-month)

2. Control Variables

When only a small number of units are randomized, as is the case of these survey data, in which 86 agencies are represented, random assignment cannot be relied on to yield treatment and control groups that are identical at baseline. The two groups may have differed initially due to chance alone. Furthermore, the attrition in our survey resulting from a few agencies dropping out of the demonstration before the patient survey was conducted and from one agency's exclusion from the analysis could have created differences between the treatment and control groups. Therefore, we could not rely on simple treatment-control differences to produce unbiased estimates of treatment impacts; rather, we had to control for possible baseline differences in the two groups. The regression models used to control for these differences also yielded coefficient estimates with lower variance

[3]We also had planned to measure the number of visits from an informal caregiver during the two weeks preceding the eight-month interview. However, an inadvertent error in the skip logic for the eight-month interview prevented us from collecting this information for all patients who had visiting caregivers.

than would a simple comparison of treatment and control means, making it more feasible to detect program impacts.

a. Description of Control Variables

We included variables in our models to control for differences between treatment and control group observations that may have existed as a result of chance or differential attrition. These control variables pertained to the episode associated with the sampled admission, the agency, and the area in which the agency is located. They drew on a number of data sources.

Episode-Level Variables. Table III.1 lists the episode-level control variables that we included in the analysis to control for possible differences in the patient mix of the treatment and the control agencies. We expected that individuals who used more non-Medicare services prior to the sampled admission would have used more of these services during and after the home health episode. Furthermore, we expected that individuals who were more severely ill, had diagnoses requiring more care, or had greater limitations in ADL would have used more services, including more non-Medicare services.

Several of the episode-level control variables drew on questions from the four-month interview. These variables measured the receipt of informal and formal non-Medicare services in the month preceding the sampled home health admission. Beneficiaries using more of these services prior to the admission may have had greater access to, or need for, them and may therefore have used more services after admission.

The other episode-level variables drew on three data sources: (1) UB-92 patient characteristics data collected to construct the demonstration's case-mix adjuster, (2) Medicare enrollment databases, and (3) Medicare Standard Analytic Files. In the remarks field for the first UB-92 bill subsequent to a demonstration admission, both treatment agencies and control agencies were required to submit

the information on patient characteristics needed to construct the 18-category Home Health Utilization Group (HHUG) case-mix adjuster. The characteristics include measures of impairment in ADL and whether a patient has certain medical conditions (in particular, cancer, diabetes, decubiti, or stroke) and certain care needs (for complex wounds). Medicare enrollment files provided us with basic patient demographic information, including the patient's age (at the start of the home health episode), gender, race, and original reason for Medicare entitlement, which is a measure of disability status. From the Standard Analytic Files, we constructed measures of Medicare service use to capture the patient's severity of illness, including measures of recent acute illness (whether admitted from a hospital and duration of previous hospital stay), longer-term home health use (for the six months preceding admission), and total Part A reimbursements (in $1,000s).

We did not have complete data from Medicare claims for the six months preceding admission for beneficiaries who were younger than age 65.5 years at admission, who were enrolled in a Medicare managed care plan, or for whom Medicare was a secondary payer during those six months. For those younger than 65.5 years of age, we used the mean value of the relevant variable for demonstration participants between 65.5 and 66 years old at admission; for those enrolled in managed care or for whom Medicare was a secondary payer, we used the overall mean. We also included in the model three binary variables (one for each of the three types of missing data) indicating that we had imputed a missing value by substituting a mean.

Agency and Area Characteristics. Table III.2 lists the agency- and area-level control variables included in the analysis. We used agency characteristics (such as size, state, and for-profit status) as control variables because different types of agencies might have had different goals, cost and management structures, and practice styles. These differences, in turn, could have affected the agencies' reliance on informal caregivers and formal non-Medicare services. For example,

TABLE III.1

EPISODE-LEVEL CONTROL VARIABLES FOR MULTIVARIATE ANALYSIS, BY SOURCE

Service Use in Month Before Sampled Admission and Caregiver Characteristics	Patient Characteristics at Episode Start	Medicare Service Use in Year Before Episode
Nursing home services[a]	Age	Length of pre-home health inpatient stay
Assisted-living facility services	Race	In skilled nursing facility within 14 days before episode start
Adult day care	Gender	Number of home health visits in six months before episode start
Home-delivered meals	Original reason for entitlement	Total Part A Medicare reimbursement in six months before episode start
Non-Medicare aide, homemaker, or maid services	Cancer	Medicaid buy in for Medicare
Informal care	Diabetes	Received Medicare for fewer than six months at episode start
Live-in informal care	Decubiti	Any HMO and any Medicare as secondary payer in six months before episode start
	Stroke	
	Needed complex wound care	
	Limitations in activities of daily living	
	Admitted to home health from hospital	
	Quarter of 1997 in which episode started	

SOURCE: Preadmission services and caregiver characteristics were obtained from the four-month patient survey. Patient characteristics at episode start were collected from the Medicare enrollment database and from the remarks field on the Uniform Billing form. Medicare service use during the pre-episode year was collected from the Medicare Standard Analytic Files.

HMO = health maintenance organization.

[a]Or a resident in a nursing home during the month preceding a hospital stay that immediately preceded a home health stay.

TABLE III.2

AGENCY- AND AREA-LEVEL CONTROL VARIABLES FOR MULTIVARIATE ANALYSES, BY SOURCE

Agency-Level Variables		Area-Level Variables
Base-Quarter Patient Service Use	Agency Characteristics	Area Characteristics
Agency practice-pattern index	Chain member Hospital based Proprietary Small agency (fewer than 30,000 visits in base year) State Rural	Physician per 10,000 (1994) Nursing home beds per 100 elderly residents (1991) Hospital occupancy rate

SOURCE: Base-quarter patient service use was obtained from the base-quarter case-mix file developed by Abt Associates, Inc. (Abt). Agency characteristics were obtained from the base-year Medicare cost reports and from the Abt enrollment file. We obtained area characteristics from the Area Resource File.

proprietary and nonprofit agencies might have had different preexisting practice patterns with respect to the number of home health visits rendered and reliance on non-Medicare services. In addition, certain types of agencies might have served a mix of patients requiring more (or less) care than the patients of other agencies, including care from informal caregivers and formal non-Medicare service providers. For example, compared with freestanding agencies, hospital-based agencies might have served somewhat more acutely ill patients.

We obtained data on the agencies' structural characteristics from base-year Medicare cost reports and from the demonstration implementation contractor, Abt. Associates, Inc. (Abt). Base-year Medicare cost reports provided information on such characteristics as for-profit status, affiliation, and agency size (as measured by total visits rendered). We defined small agencies as those that rendered fewer than 30,000 visits per year (approximately 25 percent of the sample). Large agencies were those that rendered 30,000 visits or more in the base year. During the demonstration recruitment period, Abt also constructed other measures, including whether the agency was a member of a chain and whether, according to the census definition, it was located in a rural area.

We also developed a control variable measuring each agency's predemonstration practice pattern for the full 120-day period. This variable is an index of the average number of visits per episode an agency provided during the first 120 days of base-quarter episodes relative to the average number provided by all demonstration agencies, controlling for case mix (as measured by HHUG). The index accounted for differences across agencies in the average number of visits of each type and the characteristics of the patients.[4] For example, a value of 1.10. indicates that an agency provided

[4]The specific construction is as follows. Let subscript *i* refer to the service type and subscript *j* refer to the case-mix cell. Using the case-mix adjuster (developed by Abt), we first classified an (continued...)

10 percent more visits during the 120-day period than did other demonstration agencies, controlling for differences in case mix.

We also controlled for area-level characteristics that might have influenced the care an agency rendered. For example, in rural areas, which have a limited number of nursing home beds relative to demand, hospitals may discharge to home health care some patients who otherwise would have been discharged to nursing home care. We obtained county-level characteristics from the Area Resource File (ARF), including physicians per 10,000 residents, nursing home beds per 100 elderly residents, and hospital occupancy rates. We used the most recent ARF data available for the period prior to demonstration operations. We also obtained mean Medicare reimbursement per beneficiary to capture county-level differences in overall practice patterns and health care costs.

b. Means for Control Variables

Table III.3 presents the means for the control variables in our models. To construct these means, we followed the methodology described in Chapter IV, using sample weights that gave each agency equal representation.

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[4](...continued)
agency's patients into 1 of 18 case-mix cells (j=1,..,18). Within each case-mix cell, we multiplied the average visits of each type of given agency (n_{ij}) by its national cost limit in the base year (w_i), and then summed across each of the visit types. This sum essentially reflects a weighted count of the average visits for an agency within a case-mix cell. Second, we used these weighted counts to construct the ratio of the agency's average number of visits received by patients in the case-mix cell to the average among all agencies for this ($\Sigma w_i n_{ij}$). Third, for each agency, we arrived at the practice-pattern index by summing across the 18 case-mix ratios, weighting each ratio by the agency's proportion of episodes in the case-mix cell during the base quarter (p_j). Thus, for each agency, the practice pattern is given by

$$\sum_j p_j \left(\frac{\sum_i w_i n_{ij}}{\sum_i w_i n_{ij}} \right).$$

TABLE III.3

WEIGHTED MEANS FOR CONTROL VARIABLES, BY TREATMENT STATUS

Control Variable	Treatment Group	Control Group	Treatment-Control Difference
Service Use in Month Before Sampled Admission and Caregiver Characteristics (Percent)			
In Month Before Home Health Admission, Whether Received:			
Nursing home services[a]	8.0	6.8	1.2
Assisted-living facility services	5.9	7.4	−1.5
Adult day care	4.2	3.6	0.6
Home-delivered meals	9.6	6.7	2.9*
Non-Medicare reimbursed nursing	2.2	3.9	−1.7*
Non-Medicare reimbursed aide, homemaker, or maid services	13.3	12.6	0.7
Informal care	54.8	57.4	−2.6
Live-in informal care	33.1	36.3	−3.2
Patient Characteristics at Episode Start (Percent)			
Age Original Reason for Medicare Entitlement	83.4	82.1	1.3
Age (Years)			
<65	8.7	9.5	−0.8
75 to 84	39.4	39.7	−0.3
>84	23.4	23.2	0.2
Race: White	81.8	77.9	3.9*
Female	65.4	63.4	2.0
Had			
Cancer	11.5	11.6	−0.1
Diabetes	19.7	19.9	−0.2
Decubiti (stage 3 or 4)	3.2	3.5	−0.3
Stroke	15.3	12.8	2.5
Needed Complex Wound Care	6.2	7.4	−1.2
Needed Help:			
Bathing	71.5	69.4	2.1
Eating	26.2	24.5	1.7
Dressing	61.2	61.8	−0.6
Toileting	35.0	33.0	2.0
Transferring	50.4	48.3	2.1

TABLE III.3 *(continued)*

Control Variable	Treatment Group	Control Group	Treatment-Control Difference
Admitted to Home Health from Hospital	41.3	39.8	1.5
Quarter of Episode Start			
Second quarter 1997	50.4	49.7	0.7
Third quarter 1997	24.9	19.9	5.0*
Medicare Service Use in Year Before Episode			
Length of Hospital Stay Ending Within 14 Days Before Episode Start (Days)	3.9	4.0	−0.1
SNF Stay Ending Within 14 Days Before Episode Start (Percent)	20.4	16.0	4.4*
Total Part A and Part B Inpatient/SNF/ Home Health/Hospice Reimbursement in Six Months Before Episode Start	12.1	11.8	0.3
Six Months Before Episode Start			
Had inpatient admission (percent)	66.0	66.0	0.0
Number of home health visits	9.3	10.8	−1.5
Had Medicare for Less than Six Months at Episode Start (Percent)	1.3	1.4	−0.1
Had Enrolled in HMO in Six Months Before Episode Start (Percent)	1.5	2.1	−0.6
Had Medicare as Secondary Payer in Six Months Before Episode Start (Percent)	3.1	1.8	1.3
Medicare Buy-In for Medicaid (Percent)	27.5	22.8	4.7*
Agency Characteristics (Percent of patients in such an agency, except as noted)			
Proprietary	46.8	51.2	−4.4*
Hospital Based	9.1	14.6	−5.5*
Chain	36.4	26.8	9.6*
Small Agency (<30,000 Visits in Base Year)	31.8	19.5	12.3*
Agency Practice-Pattern Index Mean[b]	0.9	1.1	−0.2*

TABLE III.3 *(continued)*

Control Variable	Treatment Group	Control Group	Treatment-Control Difference
Area Characteristics Measures			
Florida (Percent)	9.1	9.8	−0.7
Illinois (Percent)	21.9	13.6	8.3*
Massachusetts (Percent)	18.2	7.3	10.9*
Texas (Percent)	31.8	39.0	−7.2*
Urban (Percent)	86.4	85.4	1.0
Physicians per 10,000 Persons (Number; 1994)	22.3	21.5	0.8
Nursing Home Beds per 100 Persons >65 Years (Number)	5.0	5.2	−0.2
Hospital Occupancy Rate (1993)	63.0	60.8	2.2
Reimbursement per Medicare beneficiary (in $1,000s)	3.4	3.4	0.0
Sample Size	**1,053**	**1,019**	**n.a.**

SNF = skilled nursing facility; HMO = health maintenance organization; n.a. = not applicable.

[a]Or, in month preceding to hospital admission if admitted to home health from a hospital.

[b]An index of the case-mix-adjusted average visits received by an agency's patients during the first 120 days of base-quarter episode, relative to the average across all agencies.

*Statistically significant difference between the treatment and control groups at the 0.05 level.

After weighting observations to represent agencies equally, statistically significant treatment-control differences arose for 14 of the control variables. Because we developed 50 control variables, we expected to observe statistically significant differences (at the 0.05 level) by chance alone for about 3 of them. About half the observed significant differences in the control variables were in agency and area characteristics, for which we had only 85 distinct observations. The two groups differed significantly on all five agency characteristics examined (agency practice pattern, proprietary status, whether hospital based, membership in a chain, and size) as well as on three area characteristics (whether located in Illinois, in Massachusetts, and in Texas). We also observed statistically significant treatment-control differences for patient characteristic variables measuring preadmission receipt of home-delivered meals and of non-Medicare-reimbursed nursing services, Medicare skilled nursing facility stay in the 14 days preceding the home health admission, Medicaid buy-in for Medicare, race, and the quarter in which the admission was sampled.

Because we used models to control for treatment-control differences, none of these differences was large enough in magnitude to raise concern about bias. However, the differences do indicate the inadequacy of simple comparisons of mean outcomes for the treatment and control groups as a basis for drawing inferences about the effects of prospective payment.

IV. METHODS

To assess the impacts of prospective payment on the use of non-Medicare services, we estimated multivariate models for observations in the patient survey sample. The models predict the use of non-Medicare services as a function of whether a patient received treatment from a treatment or control agency, the patient's use of non-Medicare services immediately preceding the sampled home health admission, the patient's personal characteristics, and the characteristics of the agency and the area. The estimated impact of the payment method is given by the difference in regression-adjusted means between the treatment and control agencies. We used regression-adjusted means because they improve the precision of the estimated effects and control for both preexisting treatment-control differences and any treatment-control differences resulting from agency attrition from the demonstration.

In this chapter, we describe the statistical techniques used to estimate overall impacts and subgroup impacts and methods to weight the observations and to account for design effects. We also describe the sample of admissions used and discuss the hypothesis tests we conducted in our impact analyses.

A. STATISTICAL TECHNIQUES

We used two types of weighted regression models: (1) logistic regression, for binary dependent variables; and (2) Tobit analysis, for continuous dependent variables that had been censored at a discrete value. (For example, the number of visits by an informal caregiver in last two weeks is censored at zero.) In the primary analyses presented here, the observations are weighted to represent each agency equally. However, we also conducted analyses with the observations weighted to

represent each agency by its size (as measured by Medicare patients served). In all the analyses, we used standard errors that took into account the effects of sample clustering and weighting.

1. Models for Estimating Overall Impacts

a. Logit Model

When the outcome that we investigated was binary (for example, whether a patient received adult day care since admission), we used the logit (logistic regression) model to estimate demonstration impacts. The structure of the logit model is as follows:

$$(1) \quad \text{Probability } (Y = 1) = \frac{1}{1 + e^{(\alpha + x\beta + \delta T)}},$$

where Y is a binary outcome variable, X is a vector of control variables, T is a binary variable for treatment status that equals one for episodes rendered by treatment agencies and equals zero for episodes rendered by control agencies, α is the intercept term, β is the vector of regression coefficients on the control variables, and δ is a parameter that measures the impact of prospective payment on the outcome Y.

Given the nonlinearity of the logit model, the estimated impact of the payment method is not measured directly by the coefficient δ on the variable T for treatment status. To estimate the demonstration impact on the probability that $Y = 1$, we used the coefficient estimates from the model (α, β, and δ) to generate two predicted probabilities for each observation: one assuming that the observation belongs to the treatment group ($T = 1$), and one assuming that it belongs to the control group ($T = 0$). The impact estimate is the mean difference between these estimated probabilities. Because the statistical significance of δ indicates whether the odds that $Y = 1$ are significantly

different for the treatment and control groups, we used the *p*-value on this parameter to test our hypotheses about the differences between the treatment and control groups.

Throughout the tables of results, we present the unadjusted mean of each outcome variable for the control group as a point of reference for the estimated impact. This control group mean provides, for the outcome variable, a reasonable estimate of the mean value that might be expected to occur in the absence of the demonstration. We used this mean to assess the relative magnitude and importance of the estimated impact.

b. Tobit Model

The Tobit model is used to obtain consistent estimates of impacts on outcomes for which the dependent variable has been censored at a particular value. Variables may be censored from the left, right, or both sides; in this analysis, they have been censored from the left at a value of zero. Our variables measure the number of visits from an informal caregiver; the number of visits from a non-Medicare aide, homemaker, or maid; and the number of nursing home days.

The Tobit model assumes that there exists an unobserved, underlying index of the need for a service. If this index (Y^*) exceeds some threshold (zero in our specification), then the patient receives the needed service; if the index is less than the threshold, the patient receives no services. The model therefore consists of a probabilistic component for the likelihood of needing a particular type of service, and a linear component for the expected number of visits, conditional on having a need for services above the threshold:

$$(2) \quad Y^* = \alpha + X\beta + \delta T + \epsilon$$
$$Y = Y^* \text{ if } Y^* > 0$$
$$Y = 0 \text{ if } Y^* \leq 0,$$

where ϵ is a random disturbance term assumed to have a mean of zero (conditional on X and T) that reflects all the unobserved factors affecting Y, Y^* is defined as above, and the other variables and parameters are the same as in equation (1).

After the parameters of the Tobit model have been estimated, they are used to obtain predictions of the expected value of the outcome of interest for each observation. In our analysis, this prediction is given by the product of the probability that during the episode the patient used a given service and the amount of the service used, conditional on that amount being greater than zero. We calculated these predicted values twice: once assigning a given observation to the control group, and once assigning it to the treatment group. The mean difference between these two predicted values is our estimate of the impact of the demonstration on the outcome of interest.

2. Estimating Subgroup Impacts

We had two reasons for investigating demonstration impacts for subgroups. First, if impacts for the full sample existed, we were interested in determining whether they differed between key subgroups of agencies or patients. Second, even if we failed to find compelling evidence of overall impacts, it was important to determine whether they existed for selected subgroups.

The subgroup analysis used regression models similar to equations (1) and (2), with the sole difference that the subgroup model included additional control variables formed by interacting treatment status T with a set of binary (subgroup) variables defined from agency or patient characteristics. A given interaction term assumed a value of one if the observation belonged to the subgroup of interest *and* was a treatment admission; it assumed a value of zero otherwise.

We tested differences in impacts across subgroups defined by five agency characteristics and one patient characteristic. For the agency subgroup analysis, we estimated a single equation with the five subgroup variables interacted with treatment status. Each subgroup variable included in the

regression reflected a key agency characteristic (for example, the agency was a for-profit entity), and its corresponding (reference) group reflected the absence of this characteristic (for example, the agency was a nonprofit entity). We examined the following subgroup "pairs": (1) for-profit status or nonprofit status, (2) high-use practice patterns or low-use practice patterns, (3) small size or large size, (4) freestanding or hospital based, and (5) above or below the base-year cost limits. For the patient-subgroup analysis, we estimated an equation with the patient subgroup characteristic, whether on Medicaid, interacted with treatment status T.

To assess whether the impacts between particular subgroups differed, we examined the statistical significance of the coefficient on the corresponding interaction term. For example, suppose that the p-value for the coefficient on the interaction term between an agency's for-profit status and its treatment status is 0.04 (statistically significant from zero). This test indicates that the effect of prospective payment on that outcome measure differs significantly between for-profit and nonprofit agencies.

We also estimated the effect of prospective payment for each individual subgroup.[1] We did this by setting the indicator variable for the subgroup appropriately and then obtaining the predicted value of the outcome variable for each observation, first as if it were from a treatment agency ($T = 1$), and then as if from a control agency ($T = 0$). The mean difference between these two predicted values provided an unbiased estimate of the impact of prospective payment on the outcome Y for each subgroup. To assess whether the impact for a particular subgroup is different from zero, we examine the statistical significance of this impact estimate.

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[1]It is not always possible to identify small or moderate impacts for subgroups, because tests of statistical significance lose power as the sample size decreases. In our analysis, the smaller the size of the subgroup (all else equal), the less likely we were to reject the hypothesis that the payment method had no effect, for any given true effect size.

B. WEIGHTING AND DESIGN EFFECTS

1. Weighting

As indicated, we weighted the observations (1) to represent agencies equally, and (2) to represent them by size. In the main analyses, observations were weighted to represent agencies equally. We preferred this approach for two reasons. First, because the demonstration was implemented at the agency level (not the episode level), the agency is the behavioral unit of interest. Second, representing agencies equally ensured that the experiences of a few large agencies did not dominate the impact estimates. It was important to prevent this dominance from occurring because the largest agencies had 11 to 14 times as many patient survey observations as did the smallest agency (depending on the dependent variable and period).

For each agency *i*, we constructed the "agency equal" weight (w_i^e) as the ratio of the average number of observations per agency (n/k) to the number for the *i*th agency (n_i):

$$(3) \quad w_i^e = \frac{\left(\frac{n}{k}\right)}{(n_i)},$$

where n_i is the number of patient survey observations in agency *i*, *k* is the number of agencies, and *n* is the total number of observations for all agencies. Thus, patient observations for agencies with more (fewer) patient survey observations than the average agency were given lesser (greater) weight.

Across dependent variables, the minimum weights with agencies represented equally ranged from 2.6 to 3.0 for the four-month interview, and from 2.7 to 3.6 for the eight-month interview. For both interviews, the maximum weights were about 25 for all the dependent variables.

We believe that estimates from this agency-equal weighting approach have greater external validity than do those from the agency-size approach. However, a drawback of the former approach

was that the overall impact estimates could have been distorted by placing large weights on episodes in small agencies for which the sample size was small. For example, small agencies might have had only 10 observations in the sample and, by chance, could have had an unusually high or low mean for these cases. Nevertheless, this agency mean would receive the same weight in the calculation of the overall treatment (control) mean as the means of other agencies in that group. The problem is exacerbated for continuous dependent variables (such as the number of visits) because the sample for a small agency may include outliers (that is, observations for which the value of the dependent variable far exceeds that of most observations in the sample). Therefore, we also examined the impacts of the demonstration when giving each agency representation in the analysis that was equal to its relative size.

In agency-size weighting, patient interview observations for a given agency received a weight equal to the ratio of the agency's share of the demonstration episodes rendered during the first eight months of 1997 (roughly the survey sample intake period) to its share of the survey observations. That is, for agency i, we constructed the "agency size" weight (w_i^s) as:

$$(4) \quad w_i^s = \frac{\left(\frac{N_i}{N}\right)}{\left(\frac{n_i}{n}\right)},$$

where N_i is the number of episodes for agency i, N is the total number of episodes for all agencies, n_i is the number of patient survey observations in agency i, and n is the total number of survey observations for all agencies. Thus, observations for very large agencies received a weight greater than one, and those for small agencies received weights of less than one.

To investigate the sensitivity of our results to the weighting approach, we reestimated models of the overall impacts of per-episode payment with agencies represented by size. To the extent that the impact estimates were similar under the two weighting schemes, our reestimation strongly suggests that our results were not overly influenced by a small number of anomalous observations from agencies with few admissions. Thus, the results may be more broadly interpreted for policy purposes. Conversely, although dissimilar results under the two weighting schemes do not necessarily indicate that the main results are "incorrect," they do suggest the need for caution and for additional assessment to determine the valid inferences for policy purposes.

2. Adjusting for the Design Effect

We estimated the impacts of per-episode payment on the use of non-Medicare services for a sample of home health patients clustered in 86 agencies (with 2 small agencies combined), with agencies selected first, and their patients second. That is, the patient sample was clustered in a limited number of agencies.

Standard statistical packages would not be able to adjust the estimated variance of the parameter estimates for the fact that the sample of episodes was limited to agencies included in the first stage (that is, they do not adjust for the design effect of clustering). As a result, we were unable to use statistical tests on parameter estimates obtained from standard statistical packages to generalize the impacts of the demonstration to all home health agencies. (These tests can provide internally valid statements about the effect of prospective rate setting among the set of demonstration agencies.) Because the objective of the evaluation was to make externally valid statements about likely effects of a national program, we had to adjust our estimates for the design effect.

Adjusting for the design effect of clustering required adjusting the variance of the impact estimates reported by standard statistical packages to reflect differences in patient outcomes *between*

agencies. When estimating variance, standard packages would assume that the observations on patients are independent. However, outcomes for patients of a given agency may have been affected by that agency's practices, so that patient outcomes *between* agencies would have differed systematically. As a result, standard statistical packages would underestimate the standard error (the square root of the variance) and would overestimate the *t*-statistic of the estimate. The *p*-values of these overestimated *t*-statistics overstate the true significance level of the estimate. We adjusted the *p*-values upward to obtain true significance levels for drawing inferences about the effects of a national program.

The weighting of the data also produced a design effect. It was important to account for this effect because it reflects the relatively lower reliability of estimates obtained from small agencies, for which we had a limited number of survey observations. These effects could have been significant if the weights in the sample had been highly variable.

We used different approaches in the logit and Tobit models to adjust our estimates for the design effects of clustering and weighting. In the rest of this section, we discuss these approaches.

a. Logit Models

To estimate the logit models, we used a statistical package (SUDAAN) that adjusted the estimated variance of the parameter estimates for our having limited the sample of episodes to a sample of agencies and for the weighting of the observations. These design effect calculations assumed that the demonstration agencies were selected at random from the universe of all home health agencies, and that patients were then selected at random from each agency. The first assumption is not true, as agencies volunteered to participate in the demonstration. However, the

calculations gave us the correct effect on the estimated variance resulting from a two-stage patient selection process, with clustering of the observations.

b. Tobit Models

Because the SUDAAN software package does not estimate a Tobit model, we used a standard statistical package (SAS) to estimate the Tobit models. We then manually adjusted the *p*-values for the design effects.

We defined the design effect as the ratio of variance estimates that incorporate the between-agency variance to variance estimates that do not incorporate this component. To calculate this ratio, we used the SAS statistical package to reestimate all the logit models for binary dependent variables that originally had been estimated with SUDAAN. The SAS variance estimates become the denominator for this ratio. For the first admission samples with observations weighted to represent agencies equally, the mean ratios (across the binary dependent variables) were 1.01 and 1.07 for the four- and eight-month data, respectively. The largest ratio for any sample and weighting scheme was 1.11. These estimates indicate that the design effect for this analysis was small or trivial.

The standard error is underestimated, and the *t*-statistic overestimated, by a factor equal to the square root of the design effect. When the design effect was not trivial, we multiplied the *p*-value from Tobit estimates from the standard statistical package by the square root of our estimate of the design effect. In the tables that present estimates derived from Tobit models, we report *p*-values adjusted manually for the design effect. This practice ensures that we have not overstated the statistical significance of our estimates.

The minimal design effect in the patient survey is partly the result of our having established minimum and maximum sample sizes for the patient survey. This procedure reduced the sizes of

the weights that had to be applied to represent agencies equally and, therefore, reduced the design effect.

C. SAMPLES

We conducted the main analyses with observations on patients for whom the sampled home health admission represented the first admission during the demonstration. We designed our analysis so as to give preference to this sample, thereby minimizing differences in the characteristics of treatment and control group patients at home health admission. Prospectively paid agencies had a theoretical incentive to discharge patients more quickly than cost-reimbursed agencies. As a result, patients of prospectively paid agencies might have been more likely to be readmitted to home health, and their characteristics at admission might have differed from those of patients of cost-reimbursed agencies.

In practice, however, we found that patients of prospectively paid agencies were no more likely to be readmitted to home health care than were patients of cost-reimbursed agencies (Trenholm 2000). Therefore, we reestimated models for key dependent variables with the samples of "all admissions."

D. HYPOTHESIS TESTS FOR THE IMPACT ESTIMATES

For each outcome, we used a two-tailed *t*-statistic to test the null hypothesis that there is no difference between the regression-adjusted means for treatment and control agencies. As indicated in Chapter II, we used this test because we were unsure a priori whether per-episode prospective payment would have had a positive or negative impact on the use of non-Medicare services.

The *p*-value associated with the *t*-statistic was used to determine whether the demonstration had a measurable impact. A *p*-value of less than 0.05 indicates rejection of the null hypothesis and

provides significant statistical evidence of a demonstration impact. At this *p*-value, however, approximately five percent of independent tests would show, simply by chance, a statistically significant treatment-control difference even in the absence of a true program effect (known as Type I error). Therefore, in assessing whether a statistically significant treatment-control difference should be interpreted as a true program impact (especially a difference with a *p*-value close to 0.05), we considered whether the sign and magnitude of the predicted effect were consistent with those for related outcomes.

If true program effects were quite small, it is unlikely we would have observed statistically significant treatment-control differences given the moderate-sized samples available from the patient survey. For example, ignoring the small to trivial design effects associated with our weighted sample and the clustering of episodes within agencies, the minimum detectable difference (MDD) for the four-month patient sample of first demonstration admissions (2,072 cases) and an arbitrary binary variable with a mean of .50 is about five percentage points (or about 10 percent of the mean), using a two-tailed test at the 0.10 level and 80 percent power.[2] If there were a true impact on whether patients in the treatment group received any care from visiting informal caregivers (mean = .50), but the impact was less than five percentage points, there would have been a greater than one in five chance that our estimate of program effects would not be statistically significant, even at the 0.10 level. This finding would lead us to incorrectly conclude that there was no effect on this outcome. However, if impacts were moderate or sizeable, it is highly likely we would have detected them in our patient survey sample.

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[2] $\text{MDD} = 2.487\sigma\sqrt{(2/n)(1-R^2)}$, where σ is the standard deviation of the outcome variable, n is the number of observations in each of the groups being compared (assumed to be equal here), and R^2 is the percentage of the variance explained in the regression. We have assumed $R^2 = .20$.

V. OVERALL IMPACTS

As discussed in Chapter II, it is unclear a priori whether Medicare home health prospective payment will affect the use of non-Medicare services, and, if it did, whether that effect will be to increase or decrease the use of these services. Moreover, the direction of the effect may depend on whether the services in question are formal services or informal care from family members and friends. On the one hand, to the extent that prospectively paid agencies reduced the need for Medicare home health visits by rehabilitating patients more rapidly, the need for formal and informal non-Medicare services also may fall. On the other hand, if prospectively paid agencies refer more patients to formal non-Medicare services, or if patients and their families seek these services on their own, the use of formal non-Medicare services might increase under per-episode payment. Finally, to the extent that prospectively paid agencies persuade patients and families to take more personal responsibility for care, the use of formal services could drop, as the use of informal care increases.

A. IMPACTS ON INFORMAL CARE

Per-episode prospective payment did not affect beneficiaries' likelihood of receiving informal care during the eight months after home health admission. In addition, this payment method did not affect the amount of informal care provided during that time.

1. Likelihood of Receiving Informal Care

We find no effect on whether the patient received *any* informal care, *live-in* informal care, or *visiting* informal care between the time of the sampled home health admission and the four-month interview. During this period, about half the members of both the treatment group and the control group received informal care from a visiting caregiver, and slightly higher proportions received care

from a live-in caregiver. Overall, about 80 percent of the control group and a similar proportion of the treatment group received some informal care. Furthermore, we find no impact on the likelihood of receiving care from a visiting informal caregiver during the two weeks preceding the four-month interview. Slightly more than 40 percent of the control group and a similar proportion of the treatment group received this care. Table V.1 presents our results on receipt of informal care, with observations weighted to represent each agency equally. When we weight the sample to represent agencies by their size, we obtain similar results for these measures of informal care (Table V.2).

Similarly, when we draw on data from the eight-month survey, with observations weighted to represent agencies equally, we find no impact on the likelihood of receiving any informal care between the four- and eight-month interviews (Table V.3). The proportion of the control group receiving some informal care during this period is substantially smaller than the comparable proportion for the period between home health admission and the four-month interview (56 versus 80 percent), but there is little difference between the proportion of the treatment group and the control group receiving any informal care. Moreover, we find no impacts on the likelihood of receiving any informal care, live-in informal care, or visiting informal care during the two weeks preceding the eight-month interview. Weighting the observations for the eight-month data to represent agencies by size produces similar results (Table V.4).

We also find no evidence that prospective payment affects the proportion of beneficiaries receiving informal help with specific daily tasks. The eight-month interview provided measures of the receipt of informal care during the two weeks preceding the interview for various tasks of instrumental activities of daily living (IADL--housework, transportation, shopping, meal preparation, and medical treatments) and activities of daily living (ADL--bathing, transfer, dressing, and eating). The proportion of patients receiving informal care varies with the task, ranging from roughly 10

TABLE V.1

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Home Health Admission and Four-Month Interview (Percent)[a]			
Informal care from live-in caregiver	55.5	−0.6 (.74)	−1.1
Informal care from visiting caregiver	50.3	−1.4 (.51)	−2.8
Any informal care	80.4	−1.8 (.34)	−2.2
In Two Weeks Before Four-Month Interview			
Informal care from visiting caregiver (percent)	41.6	−0.4 (.84)	1.0
Number of visits[b]	1.6	.08 (.70)	5.0

SOURCE: Four-Month Patient Interview.

NOTE: Sample sizes vary as a result of missing values. The overall sample sizes range from 1,846 to 2,049. For the treatment group, the sample sizes range from 952 to 1,046, and for the control group, the sample sizes range from 894 to 1,008.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of some informal care. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from informal caregivers during the two-week reference period. The *p*-values have been adjusted manually to account for the effects of clustering and weighting.

TABLE V.2

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Home Health Admission and Four-Month Interview (Percent)[a]			
Informal care from live-in caregiver	58.2	-1.0 (.55)	-1.7
Informal care from visiting caregiver	50.1	-1.9 (.37)	-3.8
Any informal care	80.2	-1.7 (.28)	-2.1
In Two Weeks Before Four-Month Interview			
Informal care from visiting caregiver (percent)	20.7	0.8 (.70)	0.4
Number of visits[b]	1.4	.01*** (.007)	10.0

SOURCE: Four-Month Patient Interview.

NOTE: The sample sizes vary as a result of missing values. The overall sample sizes range from 1,846 to 2,049. For the treatment group, the sample sizes range from 952 to 1,046, and for the control group, the sample sizes range from 894 to 1,008.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of some informal care. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from informal caregivers during the two-week reference period. The *p*-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial.

***Significantly different from zero at the .01 level, two-tailed test.

TABLE V.3

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT EIGHT MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact[a] (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Four-Month and Eight-Month Interview (Percent)			
Any Informal Care	56.5	−0.4 (.88)	−0.7
In Two Weeks Before Eight-Month Interview			
Informal Care from Live-in Caregiver	24.5	1.4 (.51)	5.7
Informal Care from Visiting Caregiver	23.9	3.1 (.15)	13.0
Any Informal Care	41.2	1.0 (.69)	2.4
Informal Care for:			
Bathing	16.9	−0.9 (.63)	−5.3
Transferring	12.3	1.3 (.41)	10.6
Dressing	16.4	1.8 (.31)	11.0
Eating	10.3	3.2* (.06)	31.1
Housework	31.0	0.6 (.82)	1.9
Transportation	34.8	2.0 (.44)	5.7
Shopping	33.9	4.7 (.11)	13.9
Meal preparation	28.8	1.3 (.59)	4.5
Medical treatments	12.0	1.9 (.32)	15.8

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary as a result of missing values. The overall sample sizes range from 1,868 to 1,875. For the treatment group, the sample sizes range from 952 to 955, and for the control group, the sample sizes range from 916 to 921.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of any informal care. The *p*-values have been adjusted to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.

TABLE V.4

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT EIGHT MONTHS

(First Admission/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact[a] (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Four-Month and Eight-Month Interview (Percent)			
Any Informal Care	55.0	-2.8 (.28)	-5.1
In Two Weeks Before Eight-Month Interview			
Informal Care from Live-In Caregiver	24.7	-2.1 (.25)	-8.5
Informal Care from Visiting Caregiver	21.5	0.4 (.80)	1.9
Any Informal Care	39.5	-3.1* (.10)	-7.8
Informal Care for:			
Bathing	15.6	-2.7* (.08)	-17.3
Transferring	11.2	0.5 (.73)	4.5
Dressing	14.8	-1.1 (.44)	-7.4
Eating	9.2	1.4 (.34)	15.2
Housework	29.2	-2.0 (.29)	-6.8
Transportation	33.5	-0.1 (.93)	-0.3
Shopping	32.6	0.4 (.85)	1.2
Meal preparation	27.3	-1.7 (.39)	-6.2
Medical treatments	12.0	-1.3 (.45)	-10.8

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary as a result of missing values. The overall sample sizes range from 1,868 to 1,875. For the treatment group, the sample sizes range from 952 to 955, and for the control group, the sample sizes range from 916 to 921.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of some informal care. The *p*-values have been adjusted to account for the effects of clustering and weighting.

*Statistically different from zero at the .10 level, two-tailed test.

percent (for eating) to roughly 35 percent (for transportation). Regardless of whether we weight the observations to represent agencies equally (Table V.3) or to represent them by size, we find no impact of per-episode prospective payment on the likelihood of receiving informal care for ADL tasks or for IADL tasks.

2. Amount of Informal Care

A finding of a statistically significant treatment-control difference in our measure of the *amount* of informal care (that is, the number of informal caregiver visits provided during the two weeks preceding the four-month interview) depends on how we weight the data.[1] When observations are weighted to represent agencies equally, we find no difference between the groups in this measure (Table V.1). However, when we weight the observations to represent agencies by size, we find that patients of prospectively paid agencies received more informal caregiver visits during the two weeks preceding the four-month interview than did the patients of cost-reimbursed agencies (Table V.2). This result is driven largely by an atypically low average number of informal caregiver visits in a moderately large control agency. Because this result is not robust to weighting scheme and driven in large measure by the experience of a single agency, we do not believe that it represents an impact of per-episode payment. Figure V.1 displays our results for the number of informal caregiver visits at four months with observations weighted to represent agencies equally.

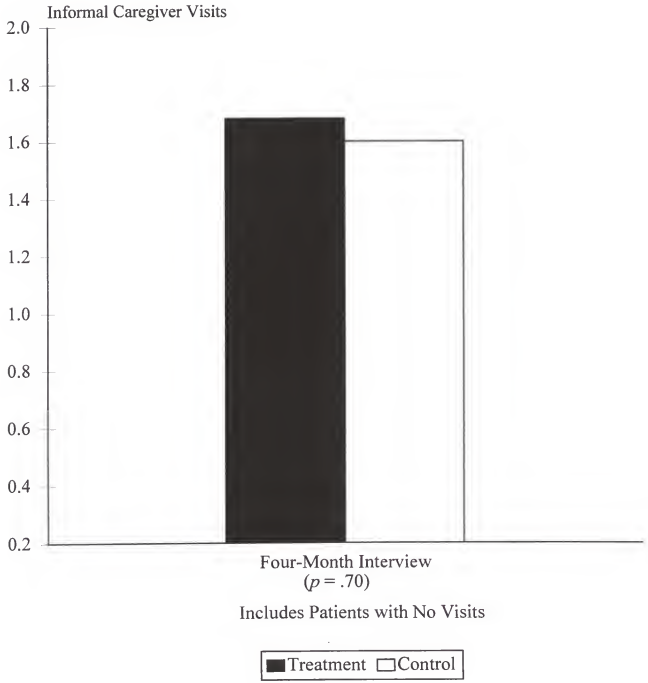
3. Sensitivity to Sample

As explained in Chapter IV, the most methodologically pure sample is limited to patients whose home health admission selected for the patient interview is also the first admission during the

[1]As indicated in Chapter IV, we do not have a measure of the number of informal caregiver visits provided during the two weeks preceding the eight-month interview.

FIGURE V.1

MEAN INFORMAL CAREGIVER VISITS, TWO WEEKS BEFORE INTERVIEW



demonstration. Prospectively paid agencies have a theoretical incentive to discharge patients more quickly than cost-reimbursed agencies. Therefore, the admission characteristics of patients in the two groups of agencies could have differed, as treatment agency patients, who were discharged more quickly, could have had a higher likelihood of readmission to home health relative to patients of cost-reimbursed agencies. We conducted most of our analyses of impacts on the use of non-Medicare services with the sample of "first admissions."

Despite the theoretical incentive, another analysis found no impact on the likelihood of a second demonstration home health admission (Trenholm 2000). By 16 months after admission, 10.7 percent of patients of prospectively paid agencies had experienced a second admission, compared with 10.2 percent of patients of cost-reimbursed agencies.

Because the likelihood of a second admission was unaffected in practice by prospective payment, we tested the sensitivity of our results to the sample used. Specifically, we reestimated our models, including models of the use of informal care, with the "all admissions" sample, which includes observations on patients selected for the patient survey at a second (or later) demonstration home health admission. This somewhat larger sample yields more precise estimates than does the first admission sample.

The results for the use of informal care obtained with the sample of all admissions are virtually identical to those obtained with the sample of first admissions (data not shown).

4. Discussion

Prospectively paid agencies reported asking patients and families to take more responsibility for care as a means of reducing the number of home health visits. Nevertheless, we do not observe any demonstration impacts on informal care.

There are several possible explanations for this discrepancy. First, treatment agencies' attempts to transfer some responsibility for care may have had no success or only limited success (patients--but not their families--may have provided more of their own care). Second, it is possible, although we believe unlikely, that greater reliance on informal care by some patients of prospectively paid agencies may have been offset by reduced reliance by others, due to the more rapid rehabilitation of the latter group. Third, increased reliance on informal caregivers by prospectively paid agencies may be part of a general trend--perhaps associated with increasing penetration of managed care organizations--that also affected patients of cost-reimbursed agencies.

Finally, it is possible that prospective payment led to increased reliance on patients and families in ways that our measures of informal care do not capture. We would not necessarily observe an impact on the likelihood of receiving any informal care if prospectively paid agencies relied more heavily on *existing* caregivers or expanded the number of caregivers in *existing* networks, relative to cost-reimbursed agencies. We would observe an increase in the receipt of this care *only* if informal caregivers were recruited for patients who would not have had informal caregivers in the absence of the demonstration. These patients represent only about 20 percent of the sample at the four-month milestone.

It is possible that agencies could more easily (and therefore less expensively) implement increased reliance on existing caregivers and expansion of existing networks than recruit informal caregivers for patients who would not otherwise have had them. However, our measures do not capture all aspects of increased reliance on existing caregivers or of expansion of existing networks. For example, they do not capture increases in the *length* of visits by existing caregivers or increases in the *number of times* a patient received assistance with a given ADL or IADL task from either existing or newly recruited informal caregivers.

B. IMPACTS ON HOME-BASED AND COMMUNITY-BASED SERVICES

We find no convincing evidence that per-episode prospective payment has an impact on the likelihood of receiving non-Medicare home-based or community-based services during the eight months after home health admission. In addition, we find no evidence that it had an impact on the amount of these services received during the period.

1. Results

We considered the impact of prospective payment on the likelihood of receiving each of four home-based and community-based services: (1) adult day care; (2) home-delivered meals; (3) non-Medicare nursing care; and (4) non-Medicare aide, homemaker, and maid services. We also considered its impact on the likelihood of receiving any of the four services, and on the number of aide, homemaker, and maid visits received during the two weeks before the interview.

We find no impacts of prospective payment on any of these measures between the time of home health admission and the four-month interview, regardless of whether the data were weighted to represent agencies equally or by size (see Tables V.5 and V.6, respectively). The proportions of patients in the treatment group and in the control group who receive a service range from roughly 3 percent for adult day care to roughly 20 percent for aide, homemaker, or maid services. Moreover, patients in both groups (including those with no visits) received an average of 0.6 to slightly more than 0.7 aide, homemaker, or maid visits during the two weeks before the four-month interview. We find no statistically significant treatment-control differences in the use of home-based and community-based services when the observations are weighted to represent agencies equally (Table V.7). With the exception of receipt of non-Medicare nursing care, slightly smaller proportions of control group members received services between the time of the four-month and eight-month interviews than between the time of home health admission and the four-month

TABLE V.5

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF HOME-BASED AND COMMUNITY-BASED NON-MEDICARE SERVICES AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (p-Value)	Impact as Percentage of Control Group Mean
Receipt Between Home Health Admission and Four-Month Interview (Percent)[a]			
Adult day care	2.7	−0.8 (.32)	−25.9
Home-delivered meals	10.4	−0.3 (.80)	−29.6
Non-Medicare reimbursed nursing care	4.0	−0.4 (.63)	−10.0
Non-Medicare reimbursed aide, homemaker, or maid care	19.1	2.3 (.18)	12.0
Any of these services	30.1	−1.4 (.45)	−4.6
In Two Weeks Before Four-Month Interview			
Number of non-Medicare reimbursed aide, homemaker, or maid visits[b]	0.60	0.1 (.30)	20.0

SOURCE: Four-Month Patient Interview.

NOTE: Sample sizes vary as a result of missing values. The overall sample sizes range from 1,980 to 2,046. For the treatment group, the sample sizes range from 953 to 1,043, and for the control group, the sample sizes range from 980 to 1,003.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of some home-based and community-based services. The p-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from aides, homemakers, or maids during the two-week reference period. The p-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial.

TABLE V.6

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF HOME-BASED AND COMMUNITY-BASED NON-MEDICARE SERVICES AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Between Home Health Admission and Four-Month Interview (Percent)[a]			
Adult day care	3.5	−0.8 (.45)	−22.9
Home-delivered meals	9.6	−1.3 (.43)	−13.5
Non-Medicare reimbursed nursing care	5.2	−0.5 (.55)	−9.6
Non-Medicare reimbursed aide, homemaker, or maid care	21.6	0.5 (.80)	2.3
Any of these services	32.3	−0.8 (.71)	−2.5
In Two Weeks Before Four-Month Interview			
Number of visits non-Medicare reimbursed aide, homemaker, or maid[b]	0.76	−0.04 (.28)	−5.3

SOURCE: Four-Month Patient Interview.

NOTE: The sample sizes vary as a result of missing values. The overall sample sizes range from 1,980 to 2,046. For the treatment group, the sample sizes range from 953 to 1,043, and for the control group, the sample sizes range from 980 to 1,003.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of some home-based and community-based services. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from aides, homemakers, or maids during the two-week reference period. The *p*-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial.

TABLE V.7

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF HOME-BASED AND COMMUNITY-BASED NON-MEDICARE SERVICES AT EIGHT MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (p-Value)	Impact as Percentage of Control Group Mean
Receipt Between Four-Month and Eight-Month Interviews (Percent)[a]			
Adult day care	2.2	-0.3 (.61)	-13.6
Home-delivered meals	8.6	1.6 (.16)	18.6
Non-Medicare reimbursed nursing care	4.3	-0.8 (.53)	-18.6
Non-Medicare reimbursed aide, homemaker, or maid care	18.6	-1.7 (.45)	-9.1
Any of these services	28.6	0.5 (.84)	1.7
In Two Weeks Before Eight-Month Interview			
Number of non-Medicare reimbursed aide, homemaker, or maid visits[b]	0.87	-0.13 (.34)	-14.9

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary as a result of missing values. The overall sample sizes range from 1,813 to 1,865. For the treatment group, the sample sizes range from 924 to 952, and for the control group, the sample sizes range from 889 to 919.

[a] These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of some home-based and community-based services. The p-values have been adjusted to account for the effects of clustering and weighting.

[b] These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from aides, homemakers, or maids during the two-week reference period. The p-values have been adjusted manually to account for the effects of clustering and weighting.

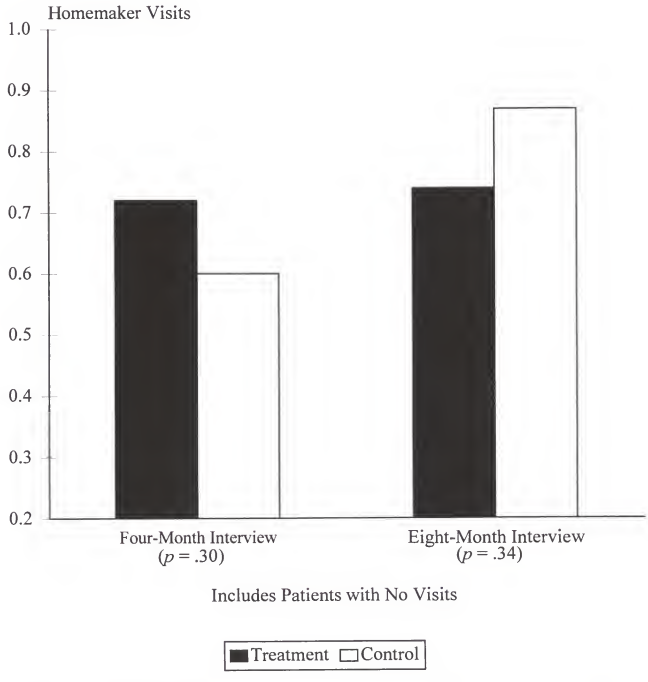
interview (compare Tables V.5 and V.7). A slightly higher proportion received non-Medicare reimbursed nursing care. However, the likelihood of treatment group patients and control group patients receiving any of these services did not differ. Control group patients received a somewhat higher average number of aide, homemaker, or maid visits during the two weeks preceding the eight-month interview than during the two weeks preceding the four-month interview. However, the number of visits received by treatment group patients and control group patients did not differ. Figure V.2 displays the results for the number of aide, homemaker, and maid visits for the two periods, illustrating the lack of effect of prospective payment.

We find statistically significant treatment-control differences for the use of home-based and community-based services only when we weighted the eight-month data to represent agencies by size (see Table V.8). We observe a negative difference in the number of aide, homemaker, and maid visits during the two weeks before the eight-month interview and a positive difference in the likelihood of receiving home-delivered meals between the four-month and eight-month interviews.

However, we do not believe that these differences represent impacts of prospective payment. First, the treatment-control difference in the *likelihood of receiving* of aide, homemaker, and maid services is very small, and it has a large *p*-value (.72). It seems unlikely that prospective payment would increase the average number of aide, homemaker, and maid visits without affecting the likelihood of receiving this service. Second, we would expect to observe one statistically significant result by chance alone, given that we tested 24 hypotheses (six measures of use of home-based and community-based services for each of two periods and two weighting schemes). Third, we do not observe a significant treatment-control difference for the four-month data for either the likelihood of receiving home-delivered meals or the number of aide, homemaker, and maid visits. Fourth, in

FIGURE V.2

MEAN NON-MEDICARE HOMEMAKER VISITS, TWO WEEKS BEFORE INTERVIEW



Note: All Medicare home health patients were sampled at first demonstration admission.

TABLE V.8

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF HOME-BASED AND COMMUNITY-BASED NON-MEDICARE SERVICES AT EIGHT MONTHS

(First Admissions/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Four-Month and Eight-Month Interviews (Percent)[a]			
Adult day care	2.1	−0.3 (.70)	−14.3
Home-delivered meals	6.4	2.4** (.05)	37.5
Non-Medicare reimbursed nursing care	4.2	−1.2 (.24)	−28.6
Non-Medicare reimbursed aide, homemaker, or maid care	19.5	−0.9 (.72)	−4.6
Any of these services	27.6	−2.1 (.48)	−7.6
In Two Weeks Before Eight-Month Interview			
Number of non-Medicare reimbursed aide, homemaker, or maid visits[b]	0.81	−0.16*** (.0001)	−19.8

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary due to missing values. The overall sample sizes range from 1,813 to 1,865. For the treatment group, the sample sizes range from 924 to 952, and for the control group, the sample sizes range from 889 to 919.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of the receipt of some home-based and community-based services. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes in which no visits were received from aides, homemakers, or maids during the two-week reference period. The *p*-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial for the eight-month samples weighted to represent agencies by size.

**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

both cases, the weighted results in Table V.7 fail to support the finding, and the differences in results with size-weighting do not appear to be artifacts of the experiences of patients at a few very large agencies.

The results for the sample of all admissions are generally similar to those for the sample of first admissions (data not shown). One additional treatment-control difference is statistically significant with the larger sample. When observations are weighted to represent agencies by size, we observe a negative treatment-control difference in the number of aide, homemaker, and maid visits during the two weeks before the four-month interview. However, the coefficients for the number of aide, homemaker, and maid visits do not even approach statistical significance when we weight observations to represent agencies equally. Therefore, we do not believe that this treatment-control difference represents an impact of per-episode payment.

2. Discussion

Although prospectively paid agencies may have had little opportunity to increase the likelihood of receipt of informal care (because most of their patients were already receiving it), that was not the case for non-Medicare home-based and community-based services. Only small minorities of patients were receiving these services at the time of home health admission (about 3 to 13 percent, depending on the service).

Thus, if prospectively paid agencies increased overall reliance on home-based and community-based services (relative to cost-reimbursed agencies), as they reported they were doing in response to per-episode payment, it seems likely that we would observe use of these services by patients who had not been receiving them previously. We offer several possible explanations for the discrepancy between the reports of prospectively paid agencies and our findings of no impacts on the use of home-based and community-based services. We believe that cost-reimbursed agencies also

increased reliance on these services as part of a general trend--perhaps associated with increasing penetration of managed care organizations. It is also possible that, as they may have done with informal care, treatment agencies may have referred more patients to home-based and community-based services, but without success. A third explanation--which we consider unlikely--is that increased reliance by some patients of prospectively paid agencies on home-based and community-based services may have been offset by reduced reliance by others, due to more rapid rehabilitation. We believe the first explanation is the most likely one.

C. IMPACTS ON RESIDENTIAL SERVICES

Per-episode prospective payment appears to reduce the use of residential services during the eight months after home health admission. We find a reduction in the likelihood of residence in assisted-living facilities during the first four months of this eight-month period and reductions in the number of days of nursing home care during the latter half.

1. Likelihood of Residence in an Assisted-Living Facility

Although only a small minority (about 10 percent) of patients of cost-reimbursed agencies were residents of assisted-living facilities between the time of home health admission and the four-month interview, we estimate that the percentage of patients of prospectively paid agencies living in such facilities was substantially smaller. When observations were weighted to represent agencies equally, we found a difference of 3.5 percentage points, a reduction of about one-third, relative to the mean for patients of cost-reimbursed agencies (Table V.9). However, when observations were weighted to represent agencies by size, the four-month treatment control difference in the likelihood of residence in an assisted-living facility, which represented a reduction of almost 30 percent, approached significance (p-value = .06; Table V.10).

TABLE V.9

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL NON-MEDICARE SERVICES AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Home Health Admission and Four-Month Interview[a]			
Resident in assisted-living facility (percent)	10.1	-3.5** (.03)	-34.7
Nursing home admission (percent)	5.0	-1.9* (.07)	-38.0
Nursing Home Days (number)[b]	2.1	-0.7 (.18)	-33.3

SOURCE: Four-Month Patient Interview.

NOTE: The sample sizes vary due to missing values. The overall sample sizes range from 2,046 to 2,063. For the treatment group, the sample sizes range from 1,042 to 1,049, and for the control group, the sample sizes range from 1,004 to 1,014.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of residence and admission. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including episodes for patients who were not nursing home residents. The *p*-values have been adjusted manually to account for the effects of clustering and weighting.

*Significantly different from zero at the .01 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.

TABLE V.10

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL NON-MEDICARE SERVICES AT FOUR MONTHS

(First Admissions/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Receipt Between Home Health Admission and Four-Month Interview[a]			
Resident in assisted-living facility (percent)	10.0	-2.8* (.06)	-28.0
Nursing home admission (percent)	5.2	-2.0** (.02)	-38.5
Nursing Home Days (number)[b]	2.3	-1.0*** (.0001)	-43.5

SOURCE: Four-Month Patient Interview.

NOTE: The sample sizes vary due to missing values. The overall sample sizes range from 2,046 to 2,063. For the treatment group, the sample sizes range from 1,042 to 1,049, and for control group, sample sizes range from 1,004 to 1,014.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of residence and admission. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including episodes for patients who were not nursing home residents. The *p*-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

When observations were weighted to represent agencies equally, the treatment-control difference for the likelihood of residence in an assisted-living facility between the time of the four-month and eight-month interviews does not reach statistical significance at the .05 level (p-value = .07); however, the difference was large in magnitude. This difference (2.6 percentage points) represents a decrease of about one-fourth relative to the mean for the control group (Table V.11).

When observations are weighted to represent agencies by size, the magnitude of the reduction in the likelihood of residing in an assisted-living facility at eight months remains quite large--with a reduction of almost 40 percent relative to the control group mean. Moreover, with size weighting, the eight-month treatment-control difference is statistically significant.

Comparison of the simple mean for the control variable measuring residence in an assisted-living facility prior to home health admission and simple mean for the dependent variable measuring residence in such a facility suggests that the proportions of patients in both the treatment group and the control group in these facilities increased between home health admission and the four-month interview and between the four-month and eight-month interviews, but that the increase was smaller for the treatment group. Specifically, 6.7 percent of control group patients and 6.4 percent of treatment group patients were residents at some time during the month before home health admission; 8.9 percent and 7.6 percent, respectively, were residents at some time between home health admission and the four-month interview; and 9.7 percent and 8.9 percent, respectively, were residents at some time between the four-month and eight-month interviews.

Despite the general increase over time in the proportion of patients residing in assisted-living facilities, some patients left these facilities, while others entered. However, given the available data, we cannot assess how prospective payment affected these transitions. Our measure of residence prior to admission captures residence in an assisted-living facility *at any time* during the month

TABLE V.11

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL NON-MEDICARE SERVICES AT EIGHT MONTHS

(First Admissions/Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Between Four-Month and Eight-Month Interview (Percent)[a]			
Resident in assisted-living facility	10.6	-2.6* (.07)	-24.5
Nursing home admission	4.2	-0.6 (.62)	-14.3
Nursing Home Days (number)[b]	6.0	-3.9*** (.002)	-65.0

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary due to missing values. The overall sample sizes range from 1,846 to 1,875. For the treatment group, sample sizes range from 934 to 955, and for the control group, sample sizes range from 912 to 920.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of residence and admission. The *p*-values are adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including episodes for which patients who were not nursing home residents. The *p*-values have been adjusted manually to account for the effects of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
*** Significantly different from zero at the .01 level, two-tailed test.

preceding admission. It is possible that some patients left assisted-living facilities before they were admitted to home health; perhaps they had entered these facilities to recuperate until they were able to return home and obtain home health care. Thus, we cannot distinguish transitions out of assisted-living facilities occurring immediately before and after home health admission.

2. Nursing Home Services

For the period between home health admission and the four-month interview, the results for nursing home admission follow a pattern somewhat similar to the pattern describing residence in an assisted-living facility. Admission to a nursing home is relatively rare. Only about five percent of patients in the cost-reimbursed group were admitted to a nursing home during that period, and we estimate that the percentage of patients of prospectively paid agencies with an admission is about 2 percentage points lower, a reduction of almost 40 percent relative to the control group. However, when observations are weighted to represent agencies equally, the estimate is not statistically significant at the .05 level (p-value = .07) (Table V.9). When we weight the observations to represent agencies by size, we obtain a coefficient of comparable magnitude, which is statistically significant (Table V.10).

Comparison of the simple mean for the control variable measuring nursing home admission prior to home health admission and of the simple mean for the dependent variable measuring admission during the four months after home health admission indicates that the proportion of control group patients and of treatment group patients in nursing homes decreased, but that the decrease was greater for the treatment group. Prior to home health admission, 5.9 percent of control group patients and 6.8 percent of treatment group patients were residents of nursing homes. Between home health admission and the four-month interview, 4.7 percent of control group patients but only 4.0 percent of treatment group patients were admitted. (Patients who were residents of nursing

homes were not eligible for a lump-sum payment; therefore patients who were in nursing homes prior to home health admission must have been discharged before qualifying.)

The results for nursing home admission between the time of the four-month and eight-month interviews are sensitive to the weighting scheme. We find no effect when observations are weighted to represent agencies equally, but a very large reduction when they are weighted to represent agencies by size. Although the patients of the few very large treatment and control agencies had substantially different rates of nursing home admission, the differences in their experiences are too small to account for a reduction in the impact estimate of the magnitude that we observe. Rates of nursing home admission were different for patients of treatment and control agencies of various sizes.

Our results for number of days in a nursing home between the time of home health admission and the four-month interview are dependent on the weighting scheme. The magnitude of the treatment-control difference is very large when observations are weighted to represent agencies equally, although the estimate does not approach statistical significance (p-value = .18). However, when observations are weighted to represent agencies by size, we find a very large, statistically significant treatment-control difference in the number of days in a nursing home (compare Tables V.9 and V.10). This treatment-control difference is not explained by differences in the experiences of the patients of the few very large agencies. In addition, between the four-month and eight-month interviews, we find a very large impact on the number of days in a nursing home, regardless of weighting scheme (see Tables V.11 and V.12). For example, when observations are weighted to represent agencies equally, we estimate that patients of prospectively paid agencies spend 3.9 fewer days in a nursing home, on average, than patients of cost-reimbursed agencies. This difference represents a reduction of almost two-thirds in the average number of nursing home days, relative to

TABLE V.12

THE IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL NON-MEDICARE SERVICES AT EIGHT MONTHS

(First Admissions/Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)	Impact as Percentage of Control Group Mean
Between Four-Month and Eight-Month Interviews (Percent)[a]			
Resident in assisted-living facility	10.1	-3.9*** (.005)	-38.6
Nursing home admission	5.6	-3.9*** (.004)	-69.6
Nursing Home Days (number)[b]	7.0	-6.8*** (.0001)	-97.1

SOURCE: Eight-Month Patient Interview.

NOTE: The sample sizes vary due to missing values. The overall sample sizes range from 1,846 to 1,875. For the treatment group, sample sizes range from 934 to 955, and for the control group, sample sizes range from 912 to 920.

[a]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of residence and admission. The *p*-values have been adjusted to account for the effects of clustering and weighting.

[b]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including episodes for patients who were not nursing home residents. The *p*-values have not been adjusted manually to account for the effects of clustering and weighting because the design effect was trivial for the eight-month samples weighted to represent agencies by size.

***Significantly different from zero at the .01 level, two-tailed test.

the mean of six days for patients of cost-reimbursed agencies. (Patients not admitted to nursing homes are included in these averages.)

Figure V.3 illustrates the impacts on nursing home days for the two four-month periods. The results on the use of residential services for the sample of all admissions are generally similar to those for the sample of first admissions (data not shown).

3. Discussion

Through what mechanism might per-episode payment reduce the use of residential services? Our data do not provide a clear-cut answer.

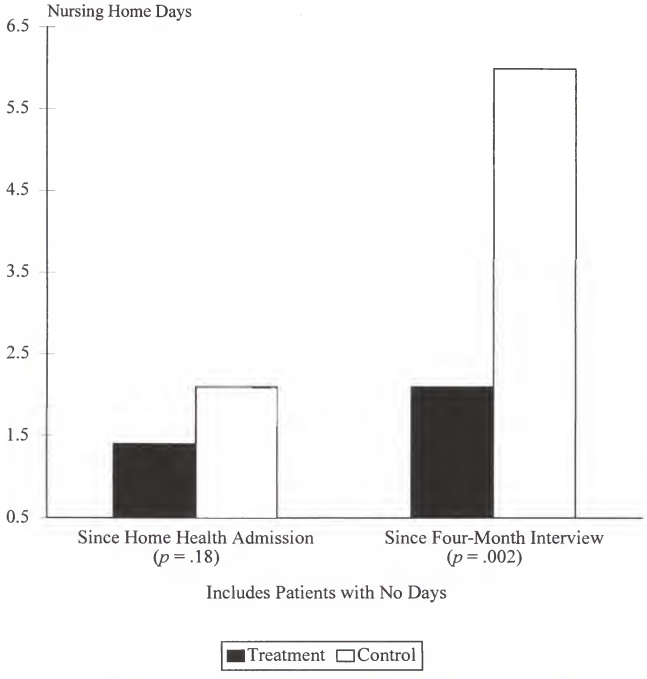
a. Are Reductions in the Use of Residential Services an Artifact of Sample Attrition?

While the overall completion rates of the four- and eight-month interviews were quite high (over 90 percent), we wanted to rule out the possibility that the observed reductions in the use of residential services were an artifact of differential sample attrition, such as potential differences in residential placement for treatment and control group members who did not respond to our interviews.

We used Medicare claims data to investigate whether sample attrition was likely affecting our survey-based impact estimates. For the four- and eight-month survey samples, we estimated the impact of prospective payment on selected measures of Medicare service use drawn from claims data. The selected measures were: (1) whether admitted to a Medicare skilled nursing facility, (2) days in a Medicare skilled nursing facility, and (3) total Medicare reimbursement. To correspond to the periods covered in the four- and eight-month interviews, we considered months one through four and months five through eight following home health admission. If sample attrition were

FIGURE V.3

MEAN NURSING HOME DAYS DURING FOUR-MONTH PERIOD



Note: All Medicare home health patients were sampled at first demonstration admission.

materially different for the treatment and control groups, we would likely observe differences between the impact estimates for the survey sample and for all demonstration participants.

While not clear-cut, our results suggest that differential sample attrition does not explain the reductions in the use of non-Medicare residential services that we observe for the survey sample. For all demonstration participants, Schore (2000) found no impacts on these measures of use of Medicare skilled nursing facilities and total Medicare reimbursement. For the survey sample, we also find no impacts on the two skilled nursing facility measures. In contrast to Schore, however, we find that total Medicare reimbursements were lower among treatment group patients in both periods. Nevertheless, since skilled nursing facilities are a type of residential service, we conclude that it is unlikely differential sample attrition explains the observed reductions in use of residential services that we observe with the survey data.

b. Do Reductions in Medical Social Work Explain Reductions in the Use of Residential Services?

Trenholm (2000) found that per-episode payment reduced the number of medical social worker visits during the first 120 days of the home health episode, but not the likelihood of receiving medical social work during that period. During months five through eight following home health admission, both the probability of receiving medical social work and the number of medical social work visits were reduced under prospective payment.

One possible explanation for our results on the use of residential services is that placement is affected by reductions in the provision of medical social work by prospectively paid home health agencies. To the extent that medical social workers suggest alternative living arrangements (when such arrangements would not otherwise have been considered), fewer medical social work visits may result in reductions in the use of residential facilities.

We investigated this possibility by first studying the relationship between Medicare medical social work and assisted-living facility residence and nursing home admission. To do so, we reestimated our models for these two dependent variables, controlling for whether the patient received medical social work during the first 120 days and 240 days of the home health episode (for the models drawing on the four-month and eight-month data, respectively). We estimated similar models controlling for the *number* of medical social worker visits during the relevant period. Neither the receipt of medical social work nor the number of medical social work visits was significantly related (at the .05 level) to residence in an assisted-living facility or to nursing home admission. Although the number of medical social work visits was related to nursing home admission in both periods at the .10 level, the estimated treatment-control differences were essentially unchanged when we included the number of medical social work visits in the models.

On the basis of this analysis, we conclude that the reduction in the use of non-Medicare residential services under per-episode payment does not work through its effect on medical social work.

c. Do Agencies' Strategies to Reduce Home Health Visits Explain Reductions in the Use of Residential Services?

Prospectively paid agencies reported more rapid rehabilitation of patients, which might, in turn, have reduced reliance on formal services, including residential services. It seems likely that prospective payment would have impacts on functional status if rehabilitation was indeed more rapid. However, analyses of the impact of per-episode payment on functional status do not indicate clear, material improvements at four or eight months after home health admission (Chen 1999). The lack of impact on functional status suggests that more rapid rehabilitation does not explain the reduction in the use of non-Medicare residential services. Prospectively paid agencies reported

asking patients and their families to take more responsibility for care, which might, in turn, have led to a reduction in the use of formal services, including residential services. However, we find no impacts on the use of informal care. Furthermore, an analysis of patient satisfaction did not find impacts on aspects of the process of care that might be associated with greater patient responsibility for self-care, such as the extent to which patients were encouraged by home health staff to be independent (Chen 1999). Conversely, Schore (1999) found that per-episode payment reduced the use of emergency room services. Better self-care and family care might explain a reduction in emergency room use if patients and their families learned to recognize signs and symptoms requiring medical intervention, and to seek that medical care before an emergency arose. We cannot rule out the possibility that greater patient and family responsibility for care explains the reduction in the use of residential services under per-episode payment.

We are unable to identify supporting evidence to show how the impacts on the use of residential services occurred. Nevertheless, the impacts that we find are too large and stable across specifications to dismiss them as statistical anomalies.

VI. SUBGROUP IMPACTS

The characteristics of the participating agencies or the characteristics of their patients may explain the differential responses of agencies to the demonstration incentives. To investigate the possibility that impacts of per-episode payment differed for subgroups of patients and agencies, we reestimated models for key dependent variables. One series of models (agency subgroup models) considered agency-level characteristics most likely to be associated with differences in agency response to the demonstration incentives. Another series (patient subgroup models) considered differences in agency response that varied according to the Medicaid status of patients, a patient characteristic of particular interest for analysis of demonstration impacts on the use of non-Medicare services.

The issue for the agency subgroup analysis is whether the pattern of results observed for the entire group of demonstration agencies is the same or different for agencies with different characteristics (for example, for-profit and nonprofit agencies). Even if we observed *no* overall impacts, we might find impacts for agencies with a given characteristic. That is, response to per-episode payment might differ by agency subgroup in such a way that the overall finding masks an impact for agencies with a specific characteristic. Even when we observe an overall impact, it is important to consider subgroup impacts because the magnitude (and even the direction) of an effect might differ by agency characteristics. A finding of impacts across most or all of the agency subgroups would strengthen the validity of the main finding, suggesting that findings may be more easily applied to agencies outside the demonstration that have characteristics differing from those of demonstration agencies. Conversely, an observed overall impact may be driven by a strong response among agencies with a specific characteristic, so we may not observe a significant impact on agencies without that characteristic.

In the patient subgroup analysis, we wanted to determine whether impacts differ for patients according to their status as Medicaid recipients. This patient subgroup is important because of concern that costs may be shifted from the Medicare program to the Medicaid program under per-episode payment.

For each agency and patient subgroup (defined in detail in Section A), we estimated program impacts on several key measures of non-Medicare service use. We then conducted statistical tests to determine (1) whether the program impact for a given subgroup was significantly different from zero; and (2) whether a significant difference existed in the impacts between the subgroup pairs (for example, between for-profit and nonprofit agencies).

A. SUBGROUP DEFINITIONS

In this section, we define the specific subgroups that were investigated. We discuss in detail our reasons for selecting particular agency and patient characteristics for the subgroup analyses.

1. Agency Characteristics

The five agency-level characteristics that we examined were the agencies' (1) for-profit status, (2) predemonstration practice patterns, (3) size, (4) auspice, and (5) base-year costs relative to the Section 223 cost limits. From these characteristics, we defined the following subgroup pairs:

- For-profit (proprietary) or nonprofit
- "High-use" practice pattern or "low-use" practice pattern. These subgroups are defined from a case-mix-adjusted index of demonstration agencies' service provision in the base year (see Chapter III for details). High-use agencies have an index value above the median value; low-use agencies are at or below the median value.
- Small size or large size. Small agencies provided fewer than 30,000 visits in the base year; large agencies provided more than 30,000 visits.

- Auspice (that is, whether the agency was hospital based or freestanding)
- Base year costs below the Section 223 cost-limits or above the cost-limits

In theory, all these characteristics would affect an agency's incentives to reduce the number of visits under prospective payment, and differences in reductions in the number of visits might have led to different impacts on the use of non-Medicare services (see the discussion in Trenholm 2000). Indeed, Trenholm found subgroup differences for characteristics in each of these five subgroup pairs, controlling for the other characteristics. Agencies with high-use practice patterns reduced home health visits in response to prospective payment by about twice as much as did agencies with low-use practice patterns. The key factor driving the overall reduction was a significantly larger reduction in the number of home health aide visits. For-profit agencies and nonprofit agencies made generally similar reductions in visits, with the exception that nonprofit agencies reduced the number of physical therapy visits more than did for-profit agencies. Small agencies made significantly larger reductions in home health aide visits than did large agencies, but reductions in all visits were only marginally significant ($p = .10$). Although only a small number of hospital-based agencies participated in the demonstration, freestanding agencies made relatively larger reductions in home health aide visits than did hospital-based agencies, but reductions in all visits were comparable for hospital-based and freestanding agencies. Agencies with costs above the limits reduced all visits roughly twice as much as agencies with costs below the limits. These differences in reductions in the number of home health aide visits and in the number of all visits by agencies with different characteristics could have led to differences in impacts on the use of non-Medicare services by agencies with different characteristics.

2. Patient Characteristics

We examined differences in impacts on the use of non-Medicare services that varied according to patient status as a Medicaid recipient. The Medicaid program covers home health and nursing home services in all states. In some states (including some demonstration states), it also covers personal assistance, homemaker, and assisted-living services. Prospectively paid agencies might have sought these Medicaid services for patients because they reduced Medicare home health services, or patients or their families might have sought them on their own, thereby shifting costs from the Medicare program to the Medicaid program. Obviously, Medicaid services are not available to patients who are not Medicaid recipients. Thus, the use of non-Medicare services might have differed on the basis of the Medicaid status of the agencies' patients.

Our measure of Medicaid status is whether a patient was enrolled in Medicaid at the time of the demonstration home health admission.[1] This measure draws on information from the Medicare enrollment database indicating whether Medicaid was "buying" into the Medicare program for a given person at the time that the person was admitted to the demonstration home health episode. When "buy-in" applies, the Medicaid program pays deductible and copayment expenses that would otherwise be the patient's financial responsibility. Given the source of this information, we expect our measure to be quite accurate. We deliberately exclude information on Medicaid status *after* home health admission because prospective payment could induce Medicaid enrollment. Patients might enroll in Medicaid during their home health episode to obtain Medicaid services that would replace Medicare home health services that had been reduced.

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[1]In some cases, enrollment in the Medicaid program may have been sought after home health admission in order to obtain personal care or homemaker services. Analysis of any impacts on Medicaid enrollment is beyond the scope of this analysis.

The number of Medicaid recipients in the survey data in each state is as follows:

State	Number of Medicaid Recipients in Sample of First Admissions	
	Four-Month Survey	Eight-Month Survey
California	158	145
Florida	65	58
Illinois	53	49
Massachusetts	64	60
Texas	158	147

For the subgroup analysis on the use of non-Medicare services, we did not consider the three patient-level subgroups analyzed elsewhere in the evaluation (that is, the ability to take oral medications, the expected cost at admission, and access to informal caregivers at admission). We exclude these patient subgroups because we obtain the data elements needed to identify them from the quality assurance data, which is available for less than 40 percent of demonstration patients overall. Consequently, including only patients for whom quality assurance data were available would have reduced our survey sample sizes substantially, and we would have only a very small number of observations on patients of some agencies. In these circumstances, anomalous cases could have materially influenced agency means, which could affect our estimates, especially in the case of observations weighted to represent agencies equally. In our judgment, subgroup analysis is not warranted for the restricted sample for which quality assurance data is available.

B. ESTIMATED DIFFERENCES FOR AGENCY SUBGROUPS

We find no evidence of subgroup impacts, or of differences in impacts across subgroup pairs, for subgroups defined by agency characteristics. For the five agency subgroup pairs listed in Section A of this chapter, we estimated subgroup differences for key dependent variables related to

informal care, home-based and community-based (nonresidential) services, and residential services. We conducted analyses separately with four-month and eight-month interview data. Table VI.1 lists the dependent variables considered in these subgroup analyses.

We provide an overview of the subgroup analyses in this section. We do not present coefficient estimates or a detailed discussion of agency subgroup impacts or differences in impacts across subgroup pairs because we believe that few, if any, of the statistically significant treatment-control differences represent impacts of per-episode payment. We tested 250 coefficients for subgroup impacts and 125 for differences in impacts across subgroup pairs. Given this large number of tests, a nontrivial number of coefficients will be statistically significant as a result of chance alone. Moreover, an anomalous case may occasionally have a substantial effect on an agency mean for a continuous outcome measure, especially for a small agency. Because we weighted the observations in these subgroup analyses to represent agencies equally, small agency means have considerable influence on our coefficient estimates.

For informal care, we found no statistically significant treatment-control differences for any of the 10 agency subgroups (two subgroups for each of five pairs). In addition, we found no statistically significant differences in the impact estimates for any of the five subgroup pairs, with either the four-month or eight-month interview data.

We found no evidence of agency subgroup impacts for home-based and community-based care. None of the subgroup treatment-control differences obtained with the four-month interview data are statistically significant. In the analysis with the eight-month data, two treatment-control differences were statistically significant at the .05 level, but this number is smaller than would be expected by chance alone (six dependent variables, five agency subgroup pairs, and two subgroups per pair yield 60 tests). Moreover, we did not find any evidence of differences across subgroup pairs for impacts

TABLE VI.1

DEPENDENT VARIABLES FOR AGENCY SUBGROUP ANALYSES

Informal Care

Receipt Between Home Health Admission and Four-Month Interview
- Care from live-in caregiver
- Care from visiting caregiver

Number of Informal Caregiver Visits in Two Weeks Before Four-Month Interview

Between Four-Month and Eight-Month Interview
- Any informal care

In Two Weeks Before Eight-Month Interview
- Care from live-in caregiver
- Care from visiting caregiver
- Any informal care

Home-Based and Community-Based Services

Receipt Between Home Health Admission and Four-Month Interview
- Adult day care
- Home-delivered meals
- Non-Medicare nursing services
- Non-Medicare aide, homemaker, or maid services
- Any of these services

Number of Aide, Homemaker, or Maid Visits in Two Weeks Before Four-Month Interview

Between Four-Month and Eight-Month Interview
- Adult day care
- Home-delivered meals
- Non-Medicare nursing services
- Non-Medicare aide, homemaker, or maid services
- Any of these services

Number of Aide, Homemaker, or Maid Visits in Two Weeks Before Eight-Month Interview

Residential Services

Receipt Between Home Health Admission and Four-Month Interview
- Residence in assisted-living facility
- Admission to nursing home
- Number of nursing home days

Between Four-Month and Eight-Month Interview
- Residence in assisted-living facility
- Admission to nursing home
- Number of nursing home days

on home-based and community-based services. In the analyses with both the four-month and the eight-month data, we observed two statistically significant differences in subgroup impacts, approximately the number that would be expected by chance alone, given 30 pair-wise comparisons.

Our estimates indicate that the overall impacts of per-episode payment on the use of non-Medicare residential services are *not* driven by impacts for a particular subgroup of agencies. We tested differences in impacts for 30 pair-wise comparisons; none are statistically significant. Moreover, the estimated treatment-control differences for a given agency subgroup are sometimes inconsistent across periods. For example, the treatment-control difference for residence in an assisted-living facility between home health admission and the time of the four-month interview is statistically significant for the high-use practice pattern subgroup, but not for the low-use practice pattern subgroup, whereas the pattern is reversed for assisted-living residence between the time of the four-month and eight-month interviews.

We cannot rule out the possibility that per-episode payment has agency subgroup impacts on the use of non-Medicare services that are too small for us to detect with the survey sample. The effective sample size is reduced because the patient observations are clustered within agencies, and some subgroups contained a relatively small number of treatment (or control) agencies. The number of treatment or control agencies is especially limited in the subgroup of small agencies (which had only eight control agencies), hospital-based agencies (four treatment agencies and six control agencies), and agencies with costs above the limits (seven treatment agencies and six control agencies). For example, the estimated variance of the difference in impacts between small and large agencies on the likelihood of receiving home-delivered meals suggests that the difference in impacts between the two subgroup pairs would have to be about 75 percentage points or larger to be detectable with 70 percent power at the .05 level of significance. The subgroups defined by for-

profit status and practice pattern contained substantially more treatment and control agencies than did the three other subgroups.[2] As a result, we have more confidence that impacts on the use of non-Medicare services do not vary for for-profit and nonprofit agencies and for agencies with high-use and low-use practice patterns than for subgroups defined by other agency characteristics.

C. IMPACTS FOR PATIENT SUBGROUP: MEDICAID RECIPIENTS

We cautiously conclude that our overall finding of no impact on informal caregiving may mask differences in effects for patients who are Medicaid recipients and for patients who are not recipients. It appears that Medicaid patients of prospectively paid agencies may be more likely than Medicaid patients of cost-reimbursed agencies to receive informal care. However, this apparent effect applies only in the longer run, rather than during the months immediately after home health admission. We find no convincing evidence that the effects of prospective payment on the use of formal home-based and community-based services depend on the Medicaid status of an agency's patients.

The apparent reductions in the use of residential services that we observed overall are driven both by impacts for patients who are Medicaid recipients and by impacts for those who are not.

1. Informal Care

The great majority of Medicaid patients and non-Medicaid patients in both prospectively paid agencies and cost-reimbursed agencies received some informal care between home health admission and the time of the four-month interview (Table VI.2). The percentage of both groups of patients

[2]The for-profit subgroup contains 20 treatment agencies and 21 control agencies; the nonprofit subgroup contains 24 treatment agencies and 20 control agencies. The high-use practice pattern subgroup contains 18 treatment agencies and 25 control agencies; the low-use practice pattern subgroup contains 26 treatment agencies and 16 control agencies.

TABLE VI.2

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between Home Health Admission and Four-Month Interview[b]		
Any Informal Care (Percent)		
Medicaid	75.1	-1.7
Non-Medicaid	81.9	-1.9
p-value for impact difference between subgroups[c]	--	(.88)
From Live-In Caregiver (Percent)		
Medicaid	46.1	-1.8
Non-Medicaid	58.4	-1.7
p-value for impact difference between subgroups[c]	--	(.97)
From Visiting Caregiver (Percent)		
Medicaid	49.4	0.9
Non-Medicaid	50.6	-1.0
p-value for impact difference between subgroups[c]	--	(.79)
In Two Weeks Before Interview[d]		
Any Informal Caregiver Visits		
Medicaid	28.8	-2.5
Non-Medicaid	20.6	1.5
p-value for impact difference between subgroups[c]	--	(.42)

SOURCE: Four-month patient interview.

NOTE: The analysis includes only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 440 to 487; for the treatment group, the sample sizes range from 259 to 290; and for the control group, the sample sizes range from 181 to 202. For the non-Medicaid group, the overall sample sizes range from 1,406 to 1,566; for the treatment group, the sample sizes range from 693 to 760; and for the control group, the sample sizes range from 713 to 806.

TABLE VI.2 *(continued)*

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are statistically different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for clustering and weighting.

receiving this care fell during the next four months (between the time of the four-month and eight-month interviews; see Table VI.3). However, the change over time in the percentage receiving informal care is more dramatic for non-Medicaid patients in the control group (82 to 55 percent) than for Medicaid patients in the control group (75 to 61 percent). The percentage of control group patients receiving a visit from an informal caregiver during the two weeks before the interviews changed little over time.

The likelihood of receiving informal care does not appear to vary by Medicaid status in the short run--between home health admission and the time of the four-month interview. As Table VI.2 indicates, with observations weighted to represent agencies equally, we find no Medicaid subgroup impacts for any of the dependent variables considered (receipt between home health admission and the four-month interview of any informal care, live-in informal care, and visiting informal care, and receipt of visiting informal care during the two weeks before the four-month interview). In addition, we did not find any impacts on the use of informal care between home health admission and the four-month interview when we weight the observations to represent agencies by size (Appendix Table C.1). We therefore conclude that prospective payment has no short-run impacts on informal care, either for patients who are Medicaid recipients or for those who are not.

When we draw on the eight-month interview data, and with observations weighted to represent agencies equally, we find significant effects for Medicaid patients for the receipt of any informal care between the time of the four-month and eight-month interviews and for the receipt of any informal care, live-in informal care, and visiting informal care during the two weeks before the eight-month interview (Table VI.3). For each of these dependent variables, we estimate that Medicaid patients of prospectively paid agencies receive more informal care than Medicaid patients of cost-reimbursed agencies. The magnitudes of all four treatment-control differences are large. For example, we

TABLE VI.3

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between Four-Month and Eight-Month Interviews[b]		
Any Informal Care (Percent)		
Medicaid	61.1	13.4**
Non-Medicaid	55.1	−4.9*
p-value for impact difference between subgroups[c]	--	(.003)‡
Receipt In Two Weeks Before Eight-Month Interview[b]		
From Live-In Caregiver (Percent)		
Medicaid	23.7	7.4
Non-Medicaid	24.8	−1.3
p-value for impact difference between subgroups[c]	--	(.07)
From Visiting Caregiver (Percent)		
Medicaid	27.3	9.4**
Non-Medicaid	22.9	0.4
p-value for impact difference between subgroups[c]	--	(0.05)†
Any Informal Care (Percent)		
Medicaid	45.9	10.0***
Non-Medicaid	39.9	−2.6
p-value for impact difference between subgroups[c]	--	(.03)†

SOURCE: Eight-month patient interview.

NOTE: The analysis included only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 455 to 456; for the treatment group, the sample sizes range from 263 to 264; and for the control group, the sample sizes range from 191 to 192. For the non-Medicaid group, the overall sample sizes range from 1,413 to 1,420; for the treatment group, the sample sizes range from 689 to 691; and for the control group, the sample sizes range from 724 to 729.

TABLE VI.3 *(continued)*

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are statistically different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for the effect of clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

† The subgroup difference in impacts is significantly different from zero at the .05 level, two-tailed test (adjusted for clustering and weighting).
‡ The subgroup difference in impacts is significantly different from zero at the .01 level, two-tailed test (adjusted for clustering and weighting).

estimate that Medicaid patients of prospectively paid agencies were 13.4 percentage points more likely than Medicaid patients of cost-reimbursed agencies to receive informal care between the four-month and eight-month interviews. Given that about 61 percent of Medicaid patients of cost-reimbursed agencies received informal care, a 13.4 percentage point gain represents about a 22 percent increase in the likelihood of receiving informal care.

For three of these four measures of the use of informal care, we also find statistically significant differences between the impact estimates for Medicaid patients and for non-Medicaid patients (Table VI.3). The fourth test for a difference approaches statistical significance at the .05 level (*p*-value = .07).

When we weight the data to represent agencies by size, our results still suggest that the impact of prospective payment may differ for Medicaid patients and for non-Medicaid patients, but the evidence is weaker and the pattern of results differs from that observed with agency-equal weighting. As Table VI.4 indicates, although none of the tests for the differences in the impact estimates for Medicaid and non-Medicaid patients are statistically significant at the .05 level with agency-size weighting, both the test for any informal care between the four-month and eight-month interviews and the test for live-in informal care during the two weeks before the eight-month interview are quite close to significance at the .05 level. In addition, just as for agency-equal weighting, we generally find large positive treatment-control differences for Medicaid patients and large negative differences for non-Medicaid patients.

Table VI.5 displays the estimated treatment-control differences from Tables VI.3 and VI.4, along with their *p*-values (in parentheses). This table shows that treatment-control differences for *non-Medicaid* patients are larger in magnitude with agency-size weighting than with agency-equal weighting and are statistically significant only with agency-size weighting. In contrast, the

TABLE VI.4

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between Four-Month and Eight-Month Interviews[b]		
Any Informal Care (Percent)		
Medicaid	53.0	7.6
Non-Medicaid	63.6	-5.9**
p-value for impact difference between subgroups[c]	--	(.06)
Receipt In Two Weeks Before Eight-Month Interview[b]		
From Live-In Caregiver (Percent)		
Medicaid	26.9	5.4
Non-Medicaid	24.2	-4.7***
p-value for impact difference between subgroups[c]	--	(.06)
From Visiting Caregiver (Percent)		
Medicaid	27.9	4.1
Non-Medicaid	20.0	-1.2
p-value for impact difference between subgroups[c]	--	(.27)
Any Informal care		
Medicaid	49.0	1.6
Non-Medicaid	37.2	-5.2*
p-value for impact difference between subgroups[c]	-	(.26)

SOURCE: Eight-month patient interview.

NOTE: The analysis included only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 455 to 456; for the treatment group, the sample sizes range from 263 to 264; and for the control group, the sample sizes range from 191 to 192. For the non-Medicaid group, the overall sample sizes range from 1,413 to 1,420; for the treatment group, the sample sizes range from 689 to 691; and for the control group, the sample sizes range from 724 to 729.

TABLE VI.4 *(continued)*

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the services. In testing hypotheses that the subgroup impacts are statistically different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for the effect of clustering and weighting.

* Significantly different from zero at the .10 level, two-tailed test.
** Significantly different from zero at the .05 level, two-tailed test.
*** Significantly different from zero at the .01 level, two-tailed test.

TABLE VI.5

ESTIMATED TREATMENT-CONTROL DIFFERENCES FOR LIKELIHOOD OF RECEIPT OF INFORMAL CARE AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS AND WEIGHTING SCHEME

	Medicaid		Non-Medicaid	
Dependent Variable	Agency Equal	Agency Size	Agency Equal	Agency Size
Between Four-Month and Eight-Month Interviews (Percent)				
Any Informal Care	13.4***	7.6	-4.9*	-5.9**
	(.003)	(.12)	(.06)	(.04)
In Two Weeks Before Eight-Month Interview (Percent)				
From Live-In Caregiver	7.4**	5.4	-1.3	-4.7**
	(.04)	(.19)	(.52)	(.05)
From Visiting Caregiver	9.4**	4.1	0.4	-1.2
	(.01)	(.30)	(.86)	(.63)
Any Informal Care	10.0**	1.6	-2.6	-5.2*
	(.02)	(.75)	(.33)	(.06)

SOURCE: Eight-Month Patient Interview.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

differences for *Medicaid* patients are smaller in magnitude with agency-size weighting than with agency-equal weighting and are statistically significant only for agency-equal weighting. The sensitivity of some of our results to weighting scheme (as well as the fact that we find no impacts on use of informal care at four months) leads us to temper our conclusions with caution.

Nevertheless, these results do not appear to be a statistical fluke. Rather, the evidence suggests that prospective payment has different impacts on informal caregiving for Medicaid patients and for non-Medicaid patients.

2. Home-Based and Community-Based Services

Non-Medicaid patients in the control group were substantially more likely than Medicaid patients to receive non-Medicare aide, homemaker, and maid services (see Tables VI.6 and VI.7). About 21 percent of non-Medicaid patients in the control group received this service between home health admission and the four-month interview, compared with about 13 percent of Medicaid patients. The percentage of non-Medicaid patients in the control group receiving this service remained essentially unchanged between the four-month and eight-month interviews, whereas the percentage of Medicaid patients fell slightly (to 11 percent).

We find no convincing evidence that prospective payment affected the use of formal home-based and community-based services for either Medicaid or non-Medicaid patients. We observe statistically significant treatment-control differences for aide, homemaker, and maid services for Medicaid patients, but the results are not consistent over time and are not robust to weighting scheme. We therefore conclude that these results do not represent an impact of prospective payment. A significant treatment-control difference for non-Medicaid patients in the likelihood of receiving home-delivered meals is generally robust to weighting scheme, but inconsistent with the incentives

TABLE VI.6

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF FORMAL, NON-RESIDENTIAL NON-MEDICARE SERVICES AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between Home Health Admission And Four-Month Interview[b]		
Home-Delivered Meals (Percent)		
Medicaid	17.9	-1.7
Non-Medicaid	8.2	-0.1
p-value for impact difference between subgroups[c]	--	(.67)
Non-Medicare-Reimbursed Aide, Homemaker, and Maid Care (Percent)		
Medicaid	12.6	6.1**
Non-Medicaid	20.9	1.8
p-value for impact difference between subgroups[c]	--	(.22)
In Two Weeks Before Interview[d]		
Aide, Homemaker, or Maid Visits (Number)		
Medicaid	0.6	0.4*
Non-Medicaid	0.6	0.1
p-value for impact difference between subgroups[c]	--	(.09)[†]

SOURCE: Four-month patient interview.

NOTE: The analysis includes only episodes reflecting a patient's first demonstration admission. Observations are weighted to represent agencies equally.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 471 to 488; for the treatment group, the sample sizes range from 275 to 287; and for the control group, the sample sizes range from 196 to 203. For the non-Medicaid group, the overall sample sizes range from 1,509 to 1,558; for the treatment group, the sample sizes range from 725 to 756; and for the control group, the sample sizes range from 784 to 803.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

TABLE VI.6 *(continued)*

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.

[†] The subgroup difference in impacts is statistically different from zero at the .01 level, two-tailed test (adjusted manually for clustering and weighting).

TABLE VI.7

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF FORMAL, NON-RESIDENTIAL, NON-MEDICARE SERVICES AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between Four-Month and Eight-Month Interviews[b]		
Home-Delivered Meals (Percent)		
Medicaid	20.5	−3.6
Non-Medicaid	5.0	3.5**
p-value for impact difference between subgroups[c]	--	(.04)[†]
Non-Medicare-Reimbursed Aide, Homemaker, and Maid Care (Percent)		
Medicaid	11.2	4.0
Non-Medicaid	20.7	−2.5
p-value for impact difference between subgroups[c]	--	(.18)
Any Home-Based and Community-Based Service (Percent)		
Medicaid	33.9	0.5
Non-Medicaid	27.0	1.3
p-value for impact difference between subgroups[c]	--	(.91)
In Two Weeks Before Interview[d]		
Aide, Homemaker, or Maid Visits (Number)		
Medicaid	0.9	−0.001
Non-Medicaid	0.8	−0.1
p-value for impact difference between subgroups[c]	--	(.45)

SOURCE: Eight-month patient interview.

NOTE: The analysis includes only episodes reflecting a patient's first admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 429 to 452; for the treatment group, the sample sizes range from 248 to 262; and for the control group, the sample sizes range from 181 to 192; for the non-Medicaid group, the overall sample sizes range from 1,384 to 1,414; for the treatment

group, the sample sizes range from 676 to 690; and for the control group, the sample sizes range from 708 to 727.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for the effect of clustering and weighting.

[d]These estimates, obtained from Tobit models, represent treatment-control difference in the average number of visits, including episodes for which the patient received no aided, homemaker, or maid visits during the two-week period. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

**Significantly different from zero at the .05 level, two-tailed test.

† The subgroup difference in impacts is statistically different from zero at the .05 level, two-tailed (adjusted for clustering and weighting).

of the demonstration. Therefore, we cautiously conclude that the difference does not represent an impact of prospective payment.

Aide, Homemaker, and Maid Services. When we draw on the four-month interview data and weight agencies equally, we find a statistically significant treatment-control difference for Medicaid patients in the likelihood of receiving aide, homemaker, and maid care between home health admission and the four-month interview (see Table VI.6).[3] Although not statistically significant at the .05 level, the estimate for the number of such visits approaches statistical significance (p-value = .07).

However, these results are not robust to weighting scheme. When we weight the four-month observations to represent agencies by size, the treatment-control difference in the likelihood of receiving aide, homemaker, and maid services drops essentially to zero, and the coefficient for the difference in the number of these visits changes direction (see Appendix Table C.2). These size-weight results are partly driven by the experiences of the patients of a few large agencies. When we exclude the observations for patients of two very large treatment agencies and one very large control agency, we find a significant positive treatment-control difference in the number of visits to Medicaid patients, a result comparable to that for the analysis conducted with agency-equal weighting.

The results for the eight-month data under agency-equal weighting are substantially different from those for the four-month data under this weighting scheme (compare Tables V1.6 and V1.7).

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[3]Receipt of adult care and non-Medicare nursing occurred so rarely that we do not present patient subgroup estimates for these dependent variables. For example, fewer than 10 Medicaid patients in the control group received adult day care between home health admission and the four-month interview. We also do not present subgroup impact estimates for the receipt of any home-based and community-based services, which includes these two rarely provided services, or for the receipt of home-delivered meals and non-Medicare aide, homemaker, and maid services.

For example, although still large in magnitude, the estimated coefficient for the likelihood that Medicaid patients would receive aide, homemaker, and maid services between the four-month and eight-month interviews is not statistically significant (p-value = .18).

When we weight the eight-month observations to represent agencies by size, we again find somewhat different results (compared with the four-month observations with agency-size weighting). For example, with agency-size weighting, the difference in the number of aide, homemaker, or maid visits at eight months is significant for both Medicaid patients and non-Medicaid. Neither difference was significant at four months under this weighting scheme.

The results discussed here differ widely depending on the weighting scheme used and the time period examined. Therefore, we do not believe that they represent impacts of prospective payment.

Home-Delivered Meals. The percentage of Medicaid patients in the control group receiving home-delivered meals is much higher than the percentage of non-Medicaid patients receiving these meals (Tables VI.6 and VI.7). Between home health admission and the time of the four-month interview, about 18 percent of Medicaid patients in the control group and only about 8 percent of non-Medicaid patients received home-delivered meals. This difference increased by the time of the next observation, four months later. The percentage of Medicaid patients in the control group receiving home-delivered meals between the four-month and eight-month interviews increased by two percentage points (to about 20 percent), whereas the percentage of non-Medicaid patients receiving this service fell by three percentage points (to about 5 percent).

Between home health admission and the time of the four-month interview, we find no convincing evidence that prospective payment had an impact on the receipt of home-delivered meals by non-Medicaid patients (Table VI.6). The results are not robust to weighting scheme. With agency-equal weighting, the estimated treatment-control difference in the likelihood of receiving

home-delivered meals during this period is essentially zero. With agency-size weighting, in contrast, the magnitude of this difference is large and approaches statistical significance (p-value = .09) (Appendix Table C.2). This result does not appear to be due to the experiences of the patients of the few large agencies.

Between the four-month and eight-month interviews, we find a statistically significant treatment-control difference in the likelihood that non-Medicaid patients would receive home-delivered meals and this difference is robust to the weighting scheme (Table VI.7). The difference--3.5 percentage points--represents a 70 percent increase relative to the mean for non-Medicaid patients of cost-reimbursed agencies. The impact estimates for Medicaid and non-Medicaid patients are also significantly different from one another during this period. When we weight the eight-month interview data to represent agencies by size, we find results that are generally comparable to those observed with agency-equal weighting (see Appendix Table C.3).

The pattern of these results over time is puzzling. Under prospective payment, agencies have an incentive to refer patients for receipt of home-delivered meals immediately after home health admission. We can think of no reason to explain delays of several months in the receipt of this service.

3. Residential Services

Medicaid patients and non-Medicaid patients in the control group had comparable experiences with respect to the use of residential services between home health admission and the time of the four-month interview. However, their experiences diverged somewhat between the four-month and eight-month interviews (Tables VI.8 and VI.9). Although about 10 percent of both groups were residents of assisted-living facilities between home health admission and the four-month interview,

TABLE VI.8

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL SERVICES AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Receipt Between and Home Health Admission and Four-Month Interview		
Resident in Assisted-Living Facility (Percent)[b]		
Medicaid	9.5	-3.7*
Non-Medicaid	10.3	-2.3*
p-value for impact difference between subgroups[c]	--	(.60)
Nursing Home Admission (Percent)[b]		
Medicaid	5.3	-3.4
Non-Medicaid	4.9	-1.4
p-value for impact difference between subgroups[c]	--	(.54)
Nursing Home Days (Number)[c]		
Medicaid	2.6	-2.2*
Non-Medicaid	2.0	-0.4
p-value for impact difference between subgroups[c]	--	(0.85)

SOURCE: Four-month patient interview.

NOTE: The analysis includes only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 493 to 496; for the treatment group, the sample sizes range from 286 to 288; and for the control group, the sample sizes range from 202 to 208. For the non-Medicaid group, the overall sample sizes range from 1,533 to 1,567; for the treatment group, the sample sizes range from 756 to 761; and for the control group, the sample sizes range from 797 to 806.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

TABLE VI.8 *(continued)*

[c]These *p*-values have been adjusted to account for clustering and weighting.

[d]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including days of patients who were not nursing home residents. In testing hypotheses that the subgroup impacts are different from zero, we have not adjusted for clustering and weighting because the design effect was trivial.

*Significantly different from zero at the.10 level, two-tailed test.

TABLE VI.9

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL SERVICES AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]
(Observations Weighted to Represent Agencies Equally)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Four-Month and Eight Month Interviews		
Resident in Assisted-Living Facility (Percent)[b]		
Medicaid	8.1	−1.2
Non-Medicaid	11.4	−2.1
p-value for impact difference between subgroups[c]	--	(.85)
Nursing Home Admission (Percent)[b]		
Medicaid	5.7	−0.9
Non-Medicaid	3.5	−0.7
p-value for impact difference between subgroups[c]	--	(.93)
Nursing Home Days (Number)[d]		
Medicaid	6.7	−4.8
Non-Medicaid	5.9	−3.5***
p-value for impact difference between subgroups[c]	--	(.99)

SOURCE: Eight-month patient interview.

NOTE: The analysis included only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 445 to 457; for the treatment group, the sample sizes range from 257 to 265; and for the control group, the sample sizes range from 188 to 192. For the Non-Medicaid group, the overall sample sizes range from 1,401 to 1,418; for the treatment group, the sample sizes range from 677 to 690; and for the control group, the sample sizes range from 724 to 728.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]The *p*-values have been adjusted to account for clustering and weighting.

TABLE VI.9 *(continued)*

[d]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including days of patients who were not residents of nursing homes. In testing hypotheses that the subgroup impacts are statistically different from zero, we did not adjust for clustering and weighting because the design impact was trivial.

***Significantly different from zero at the .01 level, two-tailed test.

the percentage of Medicaid patients residing in these facilities fell slightly between the four-month and eight-month interviews, to about 8 percent, whereas the percentage of non-Medicaid patients increased slightly, to 11 percent. About five percent of both groups were admitted to nursing homes between home health admission and the four-month interview. Between the four-month and eight-month interviews, the percentage of non-Medicaid patients admitted to a nursing home dropped to 3.5 percent, whereas the percentage of Medicaid patients increased slightly. In both periods, Medicaid patients had slightly more nursing home days than did non-Medicaid patients.

Because the Medicaid program is a major payer for nursing home care and covers assisted-living facility services in some demonstration states, we hypothesized that the use of residential services might differ for Medicaid and non-Medicaid patients. However, our analyses do not support this hypothesis. Tables VI.8 and VI.9 present subgroup estimates for the four-month data and eight-month data, respectively, on the use of residential services by patients who are Medicaid recipients and by those who are not, with the data weighted to represent agencies equally. None of the tests for differences in impacts by subgroup are statistically significant. Moreover, although only the treatment-control difference in the number of nursing home days received by non-Medicare patients between the four-month and eight-month interviews is statistically significant (Table VI.9), several other differences for both Medicaid patients and non-Medicaid patients are large in magnitude and approach statistical significance at the .05 level.

When we weight the data to represent agencies by size, we still find no significant differences in the impact estimates for Medicaid and non-Medicaid patients, for either the four-month or the eight-month data (Appendix Tables C.4 and C.5). Moreover, although the statistically significant coefficients differ somewhat, the overall pattern of results is comparable for both Medicaid and non-Medicaid patients.

We conclude that the overall estimates for the use of residential services are not driven by the experiences of either Medicaid or non-Medicaid patients.

D. DISCUSSION

1. Informal Care

Informal care for Medicaid recipients under Medicare home health per-episode payment apparently differed from informal care for patients who are not Medicaid recipients. It appears that Medicaid recipients who are patients of prospectively paid agencies might have been substantially more likely to receive care from informal caregivers five to eight months after home health admission than were Medicaid recipients who are patients of cost-reimbursed agencies. In contrast, non-Medicaid patients of prospectively paid agencies might have been less likely to receive informal care during this period than were non-Medicaid patients of cost-reimbursed agencies.

In trying to understand these differential impacts on informal care for Medicaid and non-Medicaid patients, it is useful to recall that the pattern of informal caregiving over time differed for Medicaid and non-Medicaid patients in the control group (Tables VI. 2 and VI.3). In the short term, between home health admission and the four-month interview, a higher percentage of non-Medicaid patients in the control group than Medicaid patients in this group received informal care (approximately 82 percent versus approximately 75 percent). However, during the months after home health admission, the percentage of patients receiving informal care decreased much more sharply for non-Medicaid patients than for Medicaid patients, so that the percentage of control group members receiving informal care between the four-month and eight-month interviews was lower for non-Medicaid patients than for Medicaid patients (approximately 55 percent versus approximately 61 percent).

The increases in the receipt of informal care by Medicaid patients do not appear to result from reductions in the use of residential services, which might give rise to the need for more care at home. The reductions in the use of residential services are larger in magnitude in the four months immediately after home health admission (when we find no impacts on the use of informal care) as several months later (when we do find impacts on informal care).

Another explanation for differential impacts on informal care for Medicaid patients and for non-Medicaid patients is that non-Medicaid patients may be less likely to respond to a reduction in Medicare home health care by relying on their families. Rather, non-Medicaid patients may use their higher incomes to purchase the assistance they need, particularly several months after home health admission, when their conditions have improved. However, if that explanation held, one would expect prospective payment to increase the use of aide, homemaker, and maid services by non-Medicaid patients several months after home health admission, but we do not observe that effect.

Still another possible explanation for the increased reliance on informal caregivers by Medicaid patients may lie in differences in the characteristics of informal caregivers of non-Medicaid and Medicaid. The former may be more likely to be employed full-time or to live at a distance from the patient. These family members may take leave from work and may visit to provide care but then return to their employment or homes after only a few days or weeks of caregiving. In contrast, Medicaid patients may be more likely to receive informal care from family members who are employed less than full-time or who live nearby. A reduction in Medicare home health services under prospective payment might be unlikely to change the long-term behavior of informal caregivers who are employed full-time and live at a distance. In contrast, informal caregivers who are unemployed or employed part-time or who live nearby may have more opportunity to provide informal care over an extended period. These caregivers might be more likely to respond to a

reduction in Medicare home health services under prospective payment by providing long-term informal care, even after the patients have been discharged from Medicare home health.

2. Home-Delivered Meals

As we have seen, our data suggest that the likelihood of receiving home-delivered meals increased several months after home health admission for non-Medicaid patients of prospectively paid agencies, relative to non-Medicaid patients of cost-reimbursed agencies. However, the likelihood of receiving home-delivered meals was unaffected in the short run, and we have little reason to believe that per-episode payment would have an effect on the receipt of home-delivered meals for non-Medicaid patients only after most of them were discharged from home health care. Prospectively paid agencies have an incentive to refer people to a home-delivered meals program *during* their home health episode, and patients and their families have an incentive to seek home-delivered meals during this time. It is possible that agencies make referrals to home-delivered meals during the episode, but that waiting lists delay receipt of this service. Nevertheless, widespread delays of four months seem unreasonably lengthy. Therefore, we doubt that the significant treatment-control difference for non-Medicaid patients between four and eight months after home health admission represents an impact of per-episode payment.

3. Residential Services

As we have seen, the use of residential services is probably reduced under Medicare home health prospective payment. Agencies seem to have reduced the number of nursing home days for non-Medicaid patients and probably reduced for the number of Medicaid patients. Moreover, they probably reduced residence in assisted-living facilities for both Medicaid and non-Medicaid patients.

The interview data are insufficient to permit identification of the source of payment for this residential care. Therefore, we cannot determine whether the use of *Medicaid services* have been affected, and, if they have, by how much. It is possible that the use of residential services funded by other public programs has been reduced.

In particular, it is possible that we are observing, at least in part, a reduction in admissions to *Medicare* nursing homes by people enrolled in both Medicare and Medicaid. Our data do not allow us to resolve this issue. In the patient interviews, we did not attempt to rule out Medicare as a payment source for nursing home care (see the discussion in Chapter III). In a separate analysis of claims data, we did not find a statistically significant overall treatment-control difference on Medicare nursing home use for the survey samples and periods covered here. Nor did Schore (2000) find a reduction in Medicare nursing home use for a much larger sample. We cannot rule out the possibility, however, that we would find a significant reduction in Medicare nursing home use among *Medicaid* recipients. It is also possible that the reduction in nursing home use is statistically significant only when nursing home care paid for by Medicare is combined with nursing home care paid for by other sources.

Nevertheless, a reduction in both assisted-living and nursing home care is consistent with an impact on *Medicaid* nursing home care. The Medicare and Medicaid programs are designed to cover different types of nursing home care. In general, the Medicare program covers short-term, subacute nursing home care, whereas Medicaid covers long-term nursing home care for chronic needs. Like Medicaid nursing home care, assisted-living facility services are intended for long-term care. Thus, an impact on assisted-living facility residence suggests that per-episode payment may be affecting long-term nursing home placement.

Although our results indicate that agencies under prospective payment probably reduced assisted-living facility residence both for Medicaid patients and overall, we cannot determine from the survey data the extent to which Medicaid is the payment source for these services. Medicaid covers assisted-living facility services in only two demonstration states (Illinois and Massachusetts), but other public programs, especially those intended for low-income people, pay for these services when received by Medicaid recipients. California and Texas have such public programs. In addition, we cannot rule out private purchases of residential services on behalf of Medicaid recipients. For example, a family might purchase assisted-living facility services on behalf of a Medicaid recipient, although we expect that situation to occur rarely.

The findings of the subgroup analysis for Medicaid patients suggest that analysis of Medicaid claims data would be valuable. It would be possible to definitively identify any cost-shifting to the Medicaid program under per-episode payment as one could unambiguously identify receipt of Medicaid services, and measure the amount and cost of services received. Moreover, about one-fourth of the demonstration sample patients are Medicaid recipients, so large samples of Medicaid recipients would be available (about 13,000 observations across the five demonstration states). It would also be feasible to conduct state-specific analyses. These analyses would be particularly interesting because Medicaid program services differ across the five demonstration states.

We had originally proposed using HCFA's Standard Medicaid Research Files (SMRF) to examine the impacts of per-episode payment on the use of Medicaid services. We were forced to abandon this effort because of delays in preparation of these SMRF files as HCFA programmers concentrated on ensuring that HCFA programs would perform properly in the year 2000. However, in light of the evidence presented here of impacts on Medicaid recipients and possible cost-shifting to the Medicaid program, HCFA may wish to consider pursuing analysis of the SMRF files when

they become available. Some of the demonstration states do not participate in SMRF program. Therefore, HCFA may wish to consider obtaining data for this analysis directly from the demonstration states.

VII. CONCLUSIONS

In this concluding chapter, we present a summary of our results. We then describe the limitations of our analysis and close our report with a discussion of the policy implications of the analysis.

A. SUMMARY OF RESULTS

1. Overall Results

For the Medicare home health population as a whole, home health per-episode prospective payment did not increase the use of non-Medicare services during the eight-month period after home health admission, by which time the great majority of patients had been discharged from home health care. During that period and for that population, we find:

- No increase in reliance on informal care
- No increase in the use formal home-based and community-based services, such as home-delivered meals and aide, homemaker, and maid services
- No increase in the use of residential facilities--that is, assisted-living facilities and nursing homes

Indeed, per-episode payment appears to have *reduced* the use of residential services. The likelihood of residing in an assisted-living facility was apparently reduced for the first four months after home health admission and may have been reduced for the entire eight-month period covered by the interview data. Prospective payment also appears to have reduced the number of days of nursing home care (including nursing home care covered by Medicare) beginning several months after home health admission. This pattern suggests that nursing home admission was delayed by a few weeks

or months; however, we have only weak evidence of a reduction in the likelihood of nursing home admission during the first months after home health admission.

The size and stability of our estimates on the use of residential services indicate that they are not statistical anomalies. However, we have no supporting evidence about the mechanism underlying a reduction in the use of residential facilities. We had hypothesized that medical social worker visits might facilitate residential placement, and that reductions in medical social work under prospective payment might explain reductions in the use of residential facilities. However, our analysis did not confirm this hypothesis. Although we have no direct evidence that patients and families assumed more responsibility for care in response to the urging of staff of prospectively paid agencies (relative to patients of cost-reimbursed agencies and their families), they may have done so. Greater self and family responsibility for care might have led, in turn, to less use of residential facilities during the months after home health admission.

2. Differential Impacts by Patient and Agency Characteristics

For Medicare home health patients who are also *Medicaid* recipients, per-episode payment may have increased the likelihood of receiving informal care (relative to Medicaid recipients who were patients of cost-reimbursed agencies). However, we temper this conclusion with caution because (1) we find no impacts for the first few months after home health admission, (2) some results for later months are sensitive to the weighting scheme used, and (3) we have no supporting evidence about the mechanism underlying this apparent impact.

We find no convincing evidence that the use of formal home-based and community-based services differed according to the Medicaid status of patients. For one time period, we find a robust, statistically significant treatment-control difference for non-Medicaid patients in the likelihood of receiving home-delivered meals. However, we cautiously conclude that this difference does not

represent an impact of prospective payment; although prospectively paid agencies have an incentive to refer patients for home-delivered meals immediately after home health admission, we observe a treatment-control difference only for a period beginning several months after admission.

Prospective payment probably reduced the use of assisted-living facilities and nursing homes for Medicaid patients. In addition, it probably reduced the use of these facilities by non-Medicaid patients.

We find no evidence that impacts on the use of non-Medicare services vary for for-profit and nonprofit agencies or for agencies with high-use and low-use practice patterns.[1]

B. LIMITATIONS OF THIS ANALYSIS

This study has important limitations. However, none of them casts doubt on the validity of the central finding that prospective payment did not cause sizeable increases in the use of non-Medicare services for the Medicare home health population as a whole.

A number of possible differences between participating agencies and nonparticipating agencies may lead our findings to overstate or understate the nationwide impacts of prospective payment. As in any study design based on voluntary participation, the agencies in this demonstration may reflect the group most able (or most willing) to respond to the incentives of the intervention. If that is the case, our demonstration impacts may overstate the actual reductions in home health visit use that would take place nationally under a similar payment system. In addition, if, relative to demonstration agencies, agencies nationwide had relatively low-use practice patterns, we would probably overstate the effects on home health visits of a nationwide program. Of course, if this

[1]A high-use agency is one that provided a higher-than-average number of home health visits per episode before the demonstration began, relative to the number provided by all demonstration agencies.

difference was reversed, our impact estimates might actually understate the effects. Different impacts on home health visits could lead to different impacts on the use of non-Medicare services.

Any national program of prospective payment would almost certainly differ from the one implemented for the demonstration. For example, agencies would not be protected from incurring nontrivial financial losses under a national system, which might lead some agencies to respond more cautiously to program incentives. Moreover, an agency's payment would almost certainly not be based on its own prior costs per episode; instead, it would likely be based on a national average. For many agencies, such as those with high-use practice patterns, this method of determining payments could actually lead to larger cuts in home health visits than were observed under the demonstration, as solvency would depend on their making substantial reductions in costs per episode. The use of non-Medicare services might be affected if reductions in these visits were larger than those observed in the demonstration.

We cannot rule out the possibility that per-episode payment has small, overall impacts on the use of non-Medicare services, which we do not have sufficient statistical power to detect. However, the power of this analysis is sufficient to detect overall impacts sufficiently large to be policy relevant.

For patient and agency subgroups, only relatively large impacts can be detected with confidence. Our sample contains only about 450 Medicaid recipients, too few to detect small differences between the treatment and control groups. For the agency subgroup analyses, the effective sample size is materially smaller than the nominal size because the patient observations for a given agency subgroup are clustered within a small number of agencies, particularly for small agencies, hospital-based agencies, and agencies over the Section 223 limits during the predemonstration year.

Although prospective payment appears to affect the use of some non-Medicare services four to eight months after home health admission, after most patients have been discharged from home health, we cannot investigate how long these effects last. We do not observe the use of non-Medicare services beyond the eight-month period covered by the two waves of patient interviews.

To provide time for prospectively paid agencies to develop and implement strategies in response to prospective payment, we did not begin selecting the patient survey sample until the second year of demonstration operations. Nonetheless, agencies may refine their strategies over time. It is possible, therefore, that our findings (and indeed any findings based on two years of experience with an intervention) may not accurately reflect the long-term effects of prospective payment.

C. POLICY IMPLICATIONS

The findings of this report provide important information on the effects of prospective payment at a time when HCFA, under mandate from the 1997 Balanced Budget Act, must design and implement a nationwide prospective payment system. We find that per-episode prospective payment does *not* materially increase the overall use of informal care, formal home-based and community-based services, or formal residential services--despite sizeable reductions in the provision of Medicare home health services. These reductions in Medicare home health may in turn result in substantial savings to the Medicare program and thus to society as a whole. The findings of this analysis indicate that *societal* savings will not be dominated by increased costs for services purchased by private individuals and provided by volunteer and public programs, including the Medicaid program, nor by increased reliance on the family members and friends of Medicare home health patients.

We cannot determine from the survey data the extent to which these apparent reductions in services to Medicaid recipients represent savings for the Medicaid program. Medicaid recipients

may have received services from public programs other than Medicaid, including Medicare-covered nursing home care and, in some of the demonstration states, state programs covering assisted-living facility services for low-income people. Analysis of Medicaid claims data would be required to assess the impact of Medicare home health prospective payment on Medicaid program costs.

Although we find no overall impact on the use of informal care, our estimates suggest that prospective payment may lead to increased reliance on informal caregivers of Medicaid recipients. This result is not conclusive, but it indicates that special attention is warranted to ensure informal caregivers of low-income beneficiaries are not unduly burdened under a national program of prospective payment, relative to informal caregivers of patients with higher incomes. An appropriate first step might be to monitor the burden on informal caregivers to determine whether the income levels of patients result in systematic differences in the amount of informal care provided, and in the private purchase of services by informal caregivers.

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APPENDIX A

ILLUSTRATIVE QUESTIONS FROM THE FOUR- AND EIGHT-MONTH INTERVIEWS ON USE OF NON-MEDICARE SERVICES

In this appendix, we illustrate the questions from the four- and eight-month interviews used in the analysis of the impact of per-episode prospective payment on the use of non-Medicare services. We do not present the full text of the questionnaires because the complexity of the alternative wording and skip logic makes them very difficult to follow. Requests for a full copy of the Computer Assisted Telephone Interviewing (CATI) instruments maybe addressed to Dr. Valerie Cheh, Mathematica Policy Research, Inc., Princeton, NJ.

In CATI, the wording of the questions may be varied according to the circumstances of the case. For example, the wording was varied to take into account the gender of the patient, whether the patient was alive, identity of the respondent (that is, whether the patient was the respondent or another was responding on his or her behalf), and elapsed time since home health admission or the previous interview.

To make this appendix easier to read, we made a number of assumptions about the circumstances of the case. First, we assumed that the patient is living, female, married, and responding on behalf of herself. In addition, we have made other assumptions that are applicable only to particular modules. These assumptions are explained preceding the module to which they apply.

For simplicity, we abstract in this appendix from the detailed formal skip logic of the CATI instruments. First, we do not indicate skip logic for responses of refused and don't know. Second, rather than present the question numbers of the next applicable question following response categories, we indicate the subset of patients to which a question applies (if it does not apply to all respondents).

As in the CATI instrument itself, material that was automatically filled by CATI, interviewer instructions, and answer categories are presented in all caps in this appendix to differentiate them from the questions and other material read to the respondent.

FOUR-MONTH INTERVIEW

A. SERVICE USE PRIOR TO HOME HEALTH EPISODE

The questions in Module A of the four-month interview on the use of non-Medicare services prior to the home health admission were used to create control variables for the analysis of impacts on the use of non-Medicare services.

In this appendix, we have assumed that the patient was not hospitalized immediately prior to home health admission. For patients who were hospitalized, alternative wording was provided for the questions in Module A (not shown here); these questions asked about the month prior to that hospital stay.

A2. Now please think about the month before you began receiving Medicare home health services on ADMISSION DATE.

Were you a resident of a nursing home at any time during that month?

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A3. During that month, did you live in a place (other than the nursing home) with staff to help you with personal care, such as eating or bathing, or with housework, laundry, meal preparation, or transportation?

PROBE: These places are sometimes called personal care homes, board and care homes, or continuing care retirement communities.

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A4. During that month, did you attend a program during the day with staff to help you with health or personal care? These programs are sometimes called adult day care centers.

PROBE: People attend part or all of the day and go home at night.

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A5. During that month, did you receive home-delivered meals from Meals-on-Wheels or a similar program?

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A6. During that month, did a nurse who was not paid by Medicare come to your home to help you? For example, a nurse from MEDICAID PROGRAM NAME, or paid by you, your family, or your private insurance.

PROBE: Did the person who came examine or treat you?

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A7. During that month, did an aide, homemaker, maid, or someone like that who was not paid by Medicare come to help you with personal care or things around the house?

PROBE: Include friends or family members only if they were paid to help you.

PROBE: For example, someone from MEDICAID PROGRAM NAME, or paid by you, your family, or your private insurance.

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A8. Now I would like to ask about help you may have received from family members or friends before you began receiving Medicare home health care from AGENCY NAME on ADMISSION DATE.

A9. During that month, did family members or friends, including your husband, help you with personal care, such as eating or bathing, with things around the house, or with transportation? Please do not include anyone who was paid to help.

PROBE: Things around the house such as housework, laundry, or meal preparation. Transportation includes trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

A10. IF HELP FROM FAMILY OR FRIENDS THAT MONTH.
During that month, did any of these family members or friends live in the same household as you?

PROBE: Please do not include anyone who was paid to help.

PROBE: Please include anyone living with you temporarily.

PROBE: The family members and friends who helped you with personal care, things around the house, or transportation.

PROBE: The month before you began receiving Medicare home health services from AGENCY NAME on ADMISSION DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C. SERVICE USE DURING HOME HEALTH EPISODE AND INFORMAL CAREGIVER CHARACTERISTICS

We present the wording for questions in Module C assuming that the patient had been discharged from the home health episode and the discharge date was known. Alternative wording was provided if the patient had not been discharged or if the discharge date was not known.

C4. The next questions are about different types of services you may have received between ADMISSION DATE and DISCHARGE DATE.

During the period you were receiving Medicare home health care, did you live in a place with staff to help you with personal care such as eating or bathing or with housework, laundry, meal preparation, or transportation?

PROBE: These places are sometimes called personal care homes, board and care homes, or continuing care retirement communities.

PROBE: Between ADMISSION DATE and DISCHARGE DATE when you were receiving Medicare home health from AGENCY NAME.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C5. During that period, did you attend a program with staff to help you with health or personal care during the day? These places are sometimes called adult day care centers.

PROBE: People go there during the day and come home at night.

PROBE: Between ADMISSION DATE and DISCHARGE DATE while you were getting Medicare home health from AGENCY NAME.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6. During that period, did you receive home-delivered meals from Meals-on-Wheels or a similar program?

PROBE: Between ADMISSION DATE and DISCHARGE DATE while you were getting Medicare home health from AGENCY NAME.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C7. During that period, did a nurse who was not paid by Medicare come to your home to help you? For example, a nurse from MEDICAID PROGRAM NAME or paid by you, your family or your private insurance.

PROBE: Did the person who came examine or treat you?

PROBE: Between ADMISSION DATE and DISCHARGE DATE while you were getting Medicare home health from AGENCY NAME.

PROBE: Was the nurse from an organization other than AGENCY NAME?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C9. During the period you were receiving Medicare home health care from AGENCY NAME, did an aide, homemaker, maid, or someone like that who was not paid by Medicare come to help you with personal care, with things around the house, or with transportation? This might be someone from MEDICAID PROGRAM NAME, a state or local program, or paid by you, your family, or your private insurance.

PROBE: Include friends or family members only if they were paid to help.

PROBE: Between ADMISSION DATE and DISCHARGE DATE while you were getting Medicare home health from agency.

PROBE: Personal care such as eating or bathing or things around the house such as housework, laundry, meal preparation, or trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C12. During the period you were receiving Medicare home health care from AGENCY NAME, did family members or friends, including your husband, help you with personal care, with things around the house, or with transportation? Please do not include anyone who was paid to help you.

PROBE: Between ADMISSION DATE and DISCHARGE DATE when you were receiving Medicare home health from AGENCY NAME.

PROBE: Personal care such as eating or bathing, things around the house such as housework, laundry, or meal preparation, or trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C14. IF INFORMAL CARE DURING PERIOD.
During that period, did any of these family members or friends who helped you live in the same household as you?

PROBE: Please do not include anyone who was paid to help you.

PROBE: Please include anyone living with you temporarily.

PROBE: Between ADMISSION DATE and DISCHARGE DATE while you were receiving Medicare home health from AGENCY NAME.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C15. IF INFORMAL CARE DURING PERIOD.
During that period, did any family members who did not live with you, come to help with personal care, things around the house, or transportation?

PROBE: Please do not include anyone who was paid to help you.

PROBE: Between ADMISSION DATE and DISCHARGE DATE when you were receiving Medicare home health from AGENCY NAME.

PROBE: Personal care such as eating or bathing, things around the house such as housework, laundry, or meal preparation, or trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E. SERVICES AFTER DISCHARGE

We present the wording for questions in Module E assuming that the patient had received home-delivered meals and informal care during the home health episode, but that he or she had not received the other non-Medicare services of interest during that time. Alternate wording was provided if a patient had reported earlier receipt of a service of interest. We illustrate that wording for home-delivered meals and informal care.

For patients who had been discharged from home health care, the questions on service use before and after discharge were combined to create measures of service use between home health admission and the four-month interview.

E1. The next questions are about services that you may have received after your last Medicare home health visit by AGENCY NAME on DISCHARGE DATE.

PROBE: The last visit for the episode of Medicare home health care that began on ADMISSION DATE.

E4. After you were discharged from Medicare home health by AGENCY NAME on DISCHARGE DATE did you live in a place with staff to help with personal care such as eating or bathing, or with housework, laundry, meal preparation, or transportation?

PROBE: These places are sometimes called retirement homes, board and care homes, or continuing care retirement communities.

PROBE: Do you live in a place like that now?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E7 After you were discharged from Medicare home health care, did you attend a program with staff to help you with health or personal care during the day?

PROBE: These programs are sometimes called adult day centers. People go there during the day and come home at night.

PROBE: After your last visit from AGENCY NAME on DISCHARGE DATE.

PROBE: Do you attend a program like that now?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E11. I understand that you received home-delivered meals during the period you were receiving Medicare home health care.

Did you also receive home-delivered meals from Meals-on-Wheels or a similar program after you were discharged by AGENCY NAME on DISCHARGE DATE?

PROBE: Do you receive home-delivered meals now?

< 1 > YES
< 0 > NO
< 7 > CORRECTION: NO MEALS DURING EPISODE
< 9 > DON'T KNOW/ REFUSED

E13. After you were discharged from Medicare home health care, did a nurse who was not paid by Medicare come to your home to help you?

PROBE: For example, a nurse form MEDICAID PROGRAM NAME or paid by you, your family, or your private insurance.

PROBE: After you last visit from AGENCY NAME on DISCHARGE DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E20. After you were discharged by AGENCY NAME from Medicare home health care on DISCHARGE DATE, did an aide, homemaker, maid, or someone like that who was not paid by Medicare come to help you?

PROBE: For example, someone from MEDICAID PROGRAM NAME or paid by you, your family, or your private insurance.

PROBE: Any aide who was not paid by Medicare and who came after your last visit from AGENCY NAME on DISCHARGE DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E24. IF AIDE CARE SINCE DISCHARGE.

Now, please think about the last two weeks. Did an aide, homemaker, or maid who was not paid by Medicare come to help you during the past two weeks?

PROBE: For example, someone from MEDICAID PROGRAM NAME or hired by you, your family, or your private insurance.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E30. I understand that your family members or friends, including your husband, helped you with personal care, things around the house, or transportation during the period you were receiving Medicare home health care.

After you were discharged by AGENCY NAME on DISCHARGE DATE, did family members or friends help you with personal care, things around the house, or transportation? Please do not include anyone who was paid to help you.

PROBE: Such as bathing or eating, housework, laundry, or meal preparation, or trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 7 > CORRECTION: NO INFORMAL CARE DURING EPISODE
< 9 > DON'T KNOW/ REFUSED

E31. IF INFORMAL CARE AFTER DISCHARGE.
Did any of the family members or friends who helped after your discharge from Medicare home health care, live in the same household as you at that time?

PROBE: After your last visit from AGENCY NAME on DISCHARGE DATE.

PROBE: Please do not include anyone who was paid to help you.

PROBE: Please include anyone living with you temporarily after your discharge.

PROBE: After your last visit from AGENCY NAME on DISCHARGE DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E32. IF INFORMAL CARE AFTER DISCHARGE.
After your discharge from Medicare home health care, did any family members or friends who did not live with you come to help you?

PROBE: After your last visit from AGENCY NAME on DISCHARGE DATE.

PROBE: Please do not include anyone who was paid to help you.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

E33. IF VISITING INFORMAL CARE SINCE DISCHARGE.
Now please think about the last two weeks. During that period, did any family member or friend who did not live with you come to help you with personal care, things around the house, or transportation?

PROBE: Personal care such as eating or bathing, things around the house like housework, laundry or meal preparation, or trips in a private car or on public transportation.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

F. NURSING HOME SERVICES

In this appendix, we assume that the patient was admitted to a nursing home once since home health admission. Similar questions were asked for up to three nursing home stays beginning after home health admission.

F2. The next questions are about nursing home services. A nursing home provides skilled nursing or therapy as well as personal care.

Have you ever been a resident of a nursing home?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

F3. IF EVER ADMITTED TO A NURSING HOME.
Have you been admitted to a nursing home since ADMISSION DATE when you began receiving Medicare home health services from AGENCY NAME?

Please include nursing home admissions after your last Medicare home health visit from AGENCY NAME on DISCHARGE DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

F4. IF NURSING HOME ADMISSION SINCE HOME HEALTH ADMISSION.
How many times have you been admitted to a nursing home since then?

PROBE: Since ADMISSION DATE, when you began receiving Medicare home health services from AGENCY NAME.

< 1 - 10 > NURSING HOME ADMISSIONS
< 99 > DON'T KNOW/ REFUSED

F5. Now, I would like to ask you about that nursing home stay.

F6. When did that nursing home stay begin?

PROBE: In what month, day, and year?

< 1 > JANUARY
< 2 > FEBRUARY
< 3 > MARCH
< 4 > APRIL
< 5 > MAY
< 6 > JUNE
< 7 > JULY
< 8 > AUGUST
< 9 > SEPTEMBER
< 10 > OCTOBER
< 11 > NOVEMBER
< 12 > DECEMBER
< 99 > DON'T KNOW/ REFUSED

F6d. ENTER DAY

< 1 - 31 >
< 98 > DON'T KNOW
< 99 > REFUSED

F6x. IF DON'T KNOW DAY ADMITTED TO A NURSING HOME
Was it the beginning, middle, or end of MONTH?

< 1 > BEGINNING OF MONTH
< 2> MIDDLE OF MONTH
< 3> END OF MONTH
< 9 > DON'T KNOW/ REFUSED

F6y. ENTER YEAR

19 < 97 - 98 >
< 9 > DON'T KNOW/ REFUSED

F7. When did that nursing home stay end?

PROBE: In what month, day, and year?

< 1 > JANUARY
< 2 > FEBRUARY
< 3 > MARCH
< 4 > APRIL
< 5 > MAY
< 6 > JUNE
< 7 > JULY
< 8 > AUGUST
< 9 > SEPTEMBER
< 10 > OCTOBER
< 11 > NOVEMBER
< 12 > DECEMBER
< 97 > STILL THERE
< 99 > DON'T KNOW/ REFUSED

F7d. ENTER DAY

< 1 - 31 >
< 98 > DON'T KNOW
< 99 > REFUSED

F7x. IF DON'T KNOW DAY DISCHARGED FROM NURSING HOME.

Was it the beginning, middle, or end of MONTH?

< 1 > BEGINNING OF MONTH
< 2 > MIDDLE OF MONTH
< 3 > END OF MONTH
< 9 > DON'T KNOW/ REFUSED

F7y. ENTER YEAR

19 < 97 - 98 >
< 9 > DON'T KNOW/ REFUSED

EIGHT-MONTH INTERVIEW

G. USE OF NON-MEDICARE SERVICES

The wording of the eight-month interview was varied depending on the services the patient received between home health admission and the four-month interview. In this appendix, we assume that the patient received home-delivered meals and informal care during that period.

C1. The next questions are about help you may have received since our last interview, about four months ago, on INTERVIEW DATE.

C2. IF RECEIPT OF INFORMAL CARE REPORTED IN FOUR-MONTH INTERVIEW.
In our last interview about four months ago, we learned that family members and friends, including your husband, had helped you with personal care, such as bathing or eating, with things around the house, or with transportation.

What about since then? Did family members or friends help you with personal care or things around the house at any time during the last four months? Please do not include anyone paid to help you.

PROBE: Things around the house such as housework, laundry, or meal preparation, or trips in a private car or on public transportation.

PROBE: Please include anyone living with you temporarily.

PROBE: Since our last interview about you on INTERVIEW DATE.

< 1 > YES
< 0 > NO
< 7 > CORRECTION: NO HELP EARLIER
< 9 > DON'T KNOW/ REFUSED

C5. IF INFORMAL CARE SINCE FOUR-MONTH INTERVIEW.
Now please think about the past two weeks. Did family members or friends help you with personal care, things around the house, or transportation during the last two weeks? Please do not include anyone who is paid to help you.

PROBE: Please include anyone living with you temporarily.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6. IF INFORMAL CARE DURING THE LAST TWO WEEKS, ALL PARTS OF QUESTION C6 ARE ASKED.

C6a. Did these family members or friends help you with bathing?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6b. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Getting in or out of bed?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6c. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Dressing?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6d. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Eating?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6e. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Housework or laundry?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6f. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Transportation for medical care?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6g. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Grocery shopping?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6h. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Meal preparation?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C6i. READ STEM IF NECESSARY.
Did these family members or friends help you with ...

...Routine medical treatments, for example, changing bandages, giving injections, as assisting with prescribed exercises?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C7. IF INFORMAL CARE DURING THE LAST TWO WEEKS.
Did any of the family members or friends who helped during the last two weeks live in the same household as you?

PROBE: The ones who helped with personal care or things around the house.

PROBE: Please do not include anyone who was paid to help you.

PROBE: Please include anyone living in the same household temporarily.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C8. IF INFORMAL CARE DURING THE LAST TWO WEEKS.
During the past two weeks, did family members or friends who did not live with you come to help you with personal care, things around the house, or transportation?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C9. IF INFORMAL CARE DURING THE LAST TWO WEEKS.
How many times did family members or friends who did not live with you come to your home to help during the past two weeks?

PROBE: Your best estimate would be fine.

< 1-96 > VISITS
< 99 > DON'T KNOW/ REFUSED

C11. At any time during the past four months, did a nurse who was not paid by Medicare come to your home to help you? For example, a nurse from MEDICAID PROGRAM NAME or paid by you, your family, or your private insurance.

PROBE: Did the person who came examine or treat you?

PROBE: Since our last interview about you on INTERVIEW DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C17. At any time during the past four months, did an aide, homemaker, maid, or someone like that who was not paid by Medicare come to your home to help you with personal care, with things around the house, or with transportation? For example, someone from MEDICAID PROGRAM NAME, a state or local program, or paid by you, your family, or your private insurance.

PROBE: Include friends or family members only if they were paid to help you.

PROBE: Personal care such as eating or bathing, things around the house such as housework, laundry, or meal preparation, or trips in a private car or on public transportation.

PROBE: Since our last interview about you on INTERVIEW DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C20. IF NON-MEDICARE AIDE SINCE FOUR-MONTH INTERVIEW.
Now please think about the last two weeks. Did an aide, homemaker, or maid who was not paid by Medicare come to your home during the past two weeks?

PROBE: Include friends or family members only if they were paid to help you.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C21. IF NON-MEDICARE AIDE DURING LAST TWO WEEKS.
How many times did an aide, homemaker, maid who was not paid by Medicare come to your home to help you during the past two weeks?

PROBE: Or someone like that?

< 1-96 > VISITS
< 97 > LIVE-IN AIDE
< 99 > DON'T KNOW/ REFUSED

C24. IF HOME-DELIVERED MEALS RECEIVED BETWEEN HOME HEALTH ADMISSION AND THE FOUR-MONTH INTERVIEW.
In our last interview about four months ago, we learned that you had received home-delivered meals from Meals-on-Wheels or a similar program.

What about since then? Did you receive home-delivered meals from Meals-on-Wheels or a similar program at any time during the past four months?

PROBE: Since our last interview about you on DATE.

< 1 > YES
< 0 > NO
< 7 > CORRECTION: NO HELP EARLIER
< 9 > DON'T KNOW/ REFUSED

C26. IF HOME-DELIVERED MEALS SINCE FOUR-MONTH INTERVIEW.
Now please think about the last two weeks. Did you receive home-delivered meals from Meals-on-Wheels or a similar program during the past two weeks?

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C28. At any time during the past four months, did you attend a program with staff to help you during the day with health or personal care? These programs are sometimes called adult day care centers.

PROBE: People go there during the day and come home at night.

PROBE: Since our last interview about you on DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

C33. At any time during the past four months, did you live in a place with staff to help you with personal care, such as eating or bathing, or with housework, laundry, or meal preparation?

PROBE: These places are sometimes called personal care homes, board and care homes, or continuing care retirement communities.

PROBE: Since our last interview about you on DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

H. NURSING HOME SERVICES

As for the four-month interview, questions were asked on up to three nursing home admissions. For this appendix, we assume that the patient was not in a nursing home a the time of the eight-month interview, but that he or she had been admitted to a nursing home once since the four-month interview.

D11. The next questions are about the use of nursing home services. A nursing home provides skilled nursing or therapy as well as personal care.

Were you admitted to a nursing home at any time since our last interview on INTERVIEW DATE about four months ago?

PROBE: Please exclude personal care homes and board and care homes.

PROBE: Since our last interview about you on DATE.

< 1 > YES
< 0 > NO
< 9 > DON'T KNOW/ REFUSED

D12. IF NURSING HOME ADMISSION SINCE FOUR-MONTH INTERVIEW.
How many times were you admitted to a nursing home since then?

PROBE: Since the stay that ended on DISCHARGE DATE.

< 1-10 > NURSING HOME ADMISSIONS
< 99 > DON'T KNOW/ REFUSED

D13. Now, I would like to ask you about that nursing home stay.

. When did that nursing home stay begin?

PROBE: In what month, day, and year?

< 1 > JANUARY
< 2 > FEBRUARY
< 3 > MARCH
< 4 > APRIL
< 5 > MAY
< 6 > JUNE
< 7 > JULY
< 8 > AUGUST
< 9 > SEPTEMBER
< 10 > OCTOBER
< 11 > NOVEMBER
< 12 > DECEMBER
< 99 > DON'T KNOW/ REFUSED

D13d. ENTER DAY

< 1 - 31 >
< 98 > DON'T KNOW
< 99 > REFUSED

D13x. IF DON'T KNOW DAY OF NURSING HOME ADMISSION.
Was it the beginning, middle, or end of MONTH?

< 1 > BEGINNING OF MONTH
< 2 > MIDDLE OF MONTH
< 3 > END OF MONTH
< 9 > DON'T KNOW/ REFUSED

D13y. ENTER YEAR

19 < 97 - 98 >
< 9 > DON'T KNOW/ REFUSED

D15. When did that nursing home stay end?

PROBE: In what month, day, and year?

< 1 > JANUARY
< 2 > FEBRUARY
< 3 > MARCH
< 4 > APRIL
< 5 > MAY
< 6 > JUNE
< 7 > JULY
< 8 > AUGUST
< 9 > SEPTEMBER
< 10 > OCTOBER
< 11 > NOVEMBER
< 12 > DECEMBER
< 97 > STILL THERE
< 99 > DON'T KNOW/ REFUSED

D15d. ENTER DAY

< 1 - 31 >
< 98 > DON'T KNOW
< 99 > REFUSED

D15x. IF DON'T KNOW TO DAY NURSING HOME STAY ENDED.
Was it the beginning, middle, or end of MONTH?

< 1 > BEGINNING OF MONTH
< 2 > MIDDLE OF MONTH
< 3 > END OF MONTH
< 9 > DON'T KNOW/ REFUSED

D15y. ENTER YEAR

19 < 97 - 98 >
< 9 > DON'T KNOW/ REFUSED

APPENDIX B:

DESIGN AND FIELDING OF THE PATIENT SURVEY

In this appendix, we describe the design and fielding of the patient survey in detail. The two key design issues concerned the timing of the interviews relative to home health admission and ensuring that the sample could support analyses with different weighting schemes. The patient survey was fielded in two waves of computer-assisted telephone interviews.

A. MAJOR DESIGN ISSUES FOR THE PATIENT SURVEY

The timing of the administration of the patient interviews was designed to capture both the short- and long-run impacts of per-episode prospective payment. To support analysis of the effects of the lump-sum payment, the four-month interview was administered shortly before the 120th day after home health admission. This survey provided "snapshot" measures of the use of non-Medicare services relatively close to the time of discharge for most patients (sometime during the first 90 days), as well as measures covering the entire four-month interval since home health admission.

The eight-month interview was designed to capture any longer-run effects of the lump-sum payment, as well as the effect of per-visit payments during the outlier period. When payment shifted from the lump-sum payment to outlier payment, agencies may have had an incentive to *increase* the number of visits rendered to patients still in their care (provided that per-visit rates were adequate to cover the marginal cost of the visits). In fact, however, agencies did not act on this incentive; they continued to reduce visits during the outlier period (Trenholm 2000). Of course, when we designed the patient survey, we did not know that they would do so.

A key design feature of the patient survey is that both waves of interviews were administered a specified number of days after *admission*, rather than at discharge or at a specified number of days after discharge. In this way, the data for both the treatment and control groups were collected at a comparable point in a spell of illness. Thus, differences in the timing of data collection cannot be confounded with the effects of the payment methodology. Given that the average duration of home

health stays decreased substantially under per-episode payment, this comparability of the timing for the treatment and control data is a major methodological advantage of the survey data.

B. FIELDING OF THE PATIENT SURVEY

Sample intake for the four-month survey began with patients admitted in January 1997 and ended with patients admitted in December 1997. The initiation of sample intake varied by agency, with intake beginning first for the agencies enrolling earliest in the demonstration.

For most agencies, sample intake continued for six months. This six-month period fell during the middle or latter part of the agencies' second demonstration year. It occurred during the middle of the second demonstration year for the last agencies to enroll, and during the latter part of the second demonstration year for the first agencies to enroll. Intake was extended to seven or eight months for the smallest agencies to help ensure that the minimum number of complete interviews was obtained. In a few cases, sampled admissions for the smallest agencies occurred early in the third year of their demonstration operations.

Administration of the telephone interviews began in early May 1997, approximately four months after sample intake began. Administration of the four-month interview continued through April 1998, and administration of the eight-month interview through August 1998.

We obtained a completion rate of more than 90 percent for both the four-month and eight-month interviews. The completion rate is the ratio of the number of completed interviews to the number of eligible cases. We learned after some interviews had been completed that the cases were ineligible. Ineligible cases are discussed in detail in Section C.

Most home health admissions during the sample intake period were identified from Palmetto Government Benefits Administration (PGBA) claims files, although some were identified by the agencies. (See Section B for a discussion of the construction of the universe from which the patient

sample was drawn.) After we received claims files--about the middle of each month--we added admissions from lists submitted by agencies and drew the sample. We then mailed advance letters to the sampled patients and asked the agencies to provide contact information (including each patient's telephone number and name of next of kin) via fax or courier, to facilitate reaching sampled patients quickly.

We usually were able to complete the four-month interview by the 120th day, as planned. Overall, 87 percent were completed by that time. Our inability to complete a minority (13 percent) of the four-month interviews until after the 120th day was not a problem, as the agencies did not alter their behavior after the at-risk period, despite a possible incentive to do so, and there was essentially no difference between the treatment and control groups in the proportion of interviews completed by the 120th day. Most of the four-month interviews that had not been completed by the 120th day were completed during the subsequent two weeks. In all, 96 percent of the interviews were completed by the 134th day after home health admission. (The timing of the administration of the eight-month interview was not critical.)

C. THE PATIENT SURVEY SAMPLE

1. Universe

The universe for the patient survey consisted of patients newly admitted to demonstration home health agencies for whom an agency was eligible to receive a lump-sum payment (in the case of treatment agencies) or would have been eligible under demonstration rules (in the case of control agencies). One group of newly admitted patients for whom an agency was *ineligible* for lump-sum payments consisted of those who had been discharged and were then readmitted either (1) fewer than 165 days after the initial admission, or (2) before a 45-day gap in home health services had occurred. Also, newly admitted patients who were enrolled in managed care organizations and those for whom

Medicare was a secondary payer were ineligible for a lump-sum payment, as were patients who only received home health supplies. Such patients were therefore ineligible for the patient survey sample.

a. Identifying Patients in the Universe

Most patients in the universe were identified from PGBA claims for admissions during the second half of the month. The time lags between admission and claims filing and processing necessitated that we draw the sample from claims three months before the fielding date. In the case of patients admitted during the *second* half of the month, we were able to draw the sample, complete all the tasks required in preparation for fielding, and begin interviewing on schedule. For patients admitted during the first half of the month, however, we did not have time to complete these tasks and begin interviewing on schedule. We have no reason to believe that patients admitted during the latter part of a given month were systematically different from patients admitted earlier in that month.

Overall, we were able to identify 78 percent of expected admissions during the second half of a month from claims in process and those for which processing was complete. However, drawing admissions only from the second half of the month was a problem for two groups of agencies because the number of patients admitted during that period was insufficient to produce the desired number of admissions. One group consisted of very small agencies for which it was necessary to select all, or almost all, admissions during the entire month in order to obtain the desired number. The other group consisted of a few larger agencies for which claims processing appeared to have lagged.

We developed a separate procedure for identifying the universe for these two groups of agencies. We asked the staff of the very small agencies to provide us with lists of patients admitted each month for the duration of the sample intake period. We asked the staff of the larger agencies to give us such

lists until we could identify a sufficient proportion of their admissions from claims. All the agencies in the two groups agreed to the requests. The monthly lists of patient admissions were data entered, and the resulting files were combined with the claims data files. Duplicate entries were eliminated before the sample was drawn.

b. Limitations of the Patient-Identification Approach

The procedures we implemented were designed to include as many newly admitted patients as possible, but the drawback was that we inadvertently collected data on patients who were not eligible for a lump-sum payment and therefore not appropriate for inclusion in the study. The expense of collecting data on these ineligible observations is the chief limitation of this approach. (As we describe in Section B.4, we subsequently dropped ineligible cases from the patient survey analysis files). Only six months or so after an admission would it have been possible to use claims to accurately identify the universe of patients eligible for a lump-sum payment. By that time, claims would have been relatively complete. Therefore, it would have been reasonable to assume that a gap in claims represented a break in service, and to apply the demonstration rules defining an episode. Moreover, after six months, the agencies would have corrected most claims submitted in error.

2. Review of Sample Design

We designed the patient survey sample to permit weighting of observations to represent agencies equally, or to represent them according to their size (measured in demonstration patient episodes), without introducing extremely large weights for some observations under either weighting scheme. Introducing these weights would have seriously reduced the precision of our estimates.

To reach our target of 2,000 completed eight-month interviews of eligible patients, we estimated that we had to complete 2,500 four-month interviews with these patients, and to draw a sample of

2,980 cases (Brown et al. 1995). A sample draw of this size was necessary because some patients selected for the patient survey would be identified later as ineligible for demonstration services. It also allowed for the usual sources of attrition, including death occurring between the four- and eight-month interviews, inability to locate the sampled patient, and refusal to participate.[1]

3. Implementation of Sample Selection

For the survey sample, we selected a set fraction of demonstration admissions to each agency still participating in the demonstration during the second demonstration year, provided that the fraction was expected to yield at least a minimum number of observations per agency and no more than a maximum number. The expected minimum per agency was 15 completed eight-month patient interviews, and the maximum was 100 completed eight-month patient interviews. The sampling fraction was calculated to yield 2,500 completed four-month interviews (and 2,000 completed eight-month interviews) during a sample intake period of six months, after taking into account the agencies for which the minimum and maximum sample sizes applied. For small agencies, the sampling fraction was increased so that the minimum sample size could be achieved. For the smallest agencies, all demonstration patients admitted during the sample intake period were selected. (We dropped one agency before sample selection began because our experience in the first demonstration year indicated that it was unlikely to have more than one or two admissions during the sample intake period.[2]) For the largest agencies, the sampling fraction was reduced so that no more than 100 completed eight-month interviews were expected.

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[1]We considered trying to identify ineligible patients between the four- and eight-month interviews but rejected this approach, on the grounds that the expense of doing so was likely to be greater than the cost of the interviews completed with ineligible patients.

[2]Three agencies had dropped out of the demonstration by the time the patient survey sample was selected.

4. Analysis Sample Size

We completed 2,699 four-month interviews, of which 174 cases were later lost to analysis. Most of the 174 cases were excluded because the patients were not eligible for lump-sum payments, or because we could not match the survey data to claims data. We excluded 67 of the 174 cases because the patient Medicare (Health Insurance Claim [HIC]) number for the sampled admission did not match the HIC number on other HCFA records, or because PGBA denied demonstration payment for the admission claim. We excluded 29 cases because their home health episode start dates occurred after our Medicare data cut-off date of August 31, 1997. We excluded 65 additional cases that had a discrepancy of more than 14 days between the date of admission to home health care according to the survey data and the date according to the Medicare claims data. Finally, 13 cases were excluded because Medicare claims data indicated the patients had not received home health visits, data used in the construction of control variables were missing from the remarks field of the Uniform Billing (UB-92) form, or because one agency had dropped out of the demonstration during the survey fielding period. After these exclusions, we were left with 2,525 completed four-month interviews (1,217 in the control group and 1,308 in the treatment group). Because the four-month interview was the source of control variables used in the eight-month analyses, the cases among the 174 for which we had completed the eight-month interview were lost to analysis.

The 2,525 observations comprised our sample of "all admissions" for the four-month interview. We also created a four-month sample of "first admissions" by excluding 453 cases (198 control group and 255 treatment group patients) who had demonstration home health admissions prior to the sampled admission. In this way, we created an analysis sample for the four-month interview of 2,072 first admissions. For reasons discussed in Chapter IV, the first-admission samples were our

main analysis samples. However, a particular analysis may have had somewhat fewer observations because that dependent variable was missing values.

We completed 2,291 eight-month interviews from the sample of 2,525 patients for which four-month data were available for analysis. These 2,291 cases (1,188 in the treatment group and 1,103 in the control group) comprised the all-admissions sample for the eight-month interview. In addition, we completed 1,883 eight-month interviews with the 2,072 patients in the four-month, first-admissions sample. These 1,883 cases (958 in the treatment group and 925 in the control group) comprised the first-admissions sample for the eight-month interview.

For both the four- and eight-month surveys, the analysis sample contained patients from 85 agencies (44 in the treatment group and 41 in the control group). A total of 86 agencies had patients in the survey analysis samples, but observations on the patients of 2 very small agencies were combined for the analysis. The two agencies were owned by the same organization, were located close to each other, and were in the treatment group. Together, they had a total of 12 patients for which four-month interview data were available and 8 patients for which we had eight-month interview data. The minimum number of cases per agency was 8 in both the four- and eight-month interviews; the maximum per agency was 96 in the four-month interview and 89 in the eight-month interview.

After the analysis was completed and a draft report prepared, we identified 112 cases that were ineligible for demonstration lump-sum payment. These were primarily cases in which Medicare was the secondary payer and cases in which the patient was a member of a particular type of managed care organization for which claims appear on Medicare files. In one case, the patient's only home health claim was for supplies. We excluded these cases and were left with a first-admission sample for the four-month interview of 1,960 cases and a first-admission sample for the eight-month

interview of 1,782 cases. We re-estimated our impact coefficients for key dependent variables and these reduced first-admission samples. As discussed in Chapter IV, there were no material differences in our estimates with these reduced samples.

APPENDIX C

SUPPLEMENTAL TABLES FOR CHAPTER VI

TABLE C.1

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF INFORMAL CARE AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]

(Observations Weighted to Represent Agencies, by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Home Health Admission and Four-Month Interview[b]		
Any Informal Care (Percent)		
Medicaid	73.4	0.8
Non-Medicaid	81.7	−2.4
p-value for impact difference between subgroups[c]	--	(.32)
From Live-In Caregiver (Percent)		
Medicaid	49.6	0.1
Non-Medicaid	60.2	−2.6
p-value for impact difference between subgroups[c]	--	(.55)
From Visiting Caregiver (Percent)		
Medicaid	45.5	4.1
Non-Medicaid	51.1	−2.6
p-value for impact difference between subgroups[c]	--	(.33)
In Two Weeks Before Interview[d]		
Any Informal Care Visits (Number)		
Medicaid	24.6	3.4
Non-Medicaid	19.8	0.8
p-value for impact difference between subgroups[c]	--	(.58)

SOURCE: Four-month patient interview.

NOTE: Observations are weighted to represent agencies by size. The analysis includes only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 440 to 487; for the treatment group, the sample sizes range from 259 to 290; and for the control group, the sample sizes range from 181 to 202. For the non-Medicaid group, the overall sample sizes range from 1,406 to 1,566; for the treatment group, the sample sizes range from 693 to 760; and for the control group, the sample sizes range from 713 to 806.

TABLE C.1 *(continued)*

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are statistically different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for clustering and weighting, except that no manual adjustment was made for informal caregiver visits, because the design effect was trivial.

[d]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including patients who received no informal caregiver visits during the two-week reference period. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting because the design effect is trivial.

TABLE C.2

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF FORMAL, NON-RESIDENTIAL NON-MEDICARE SERVICES AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]

(Observations Weighted to Represent Agencies, by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Home Health Admission and Four-Month Interview[b]		
Home-Delivered Meals (Percent)		
Medicaid	14.3	2.8
Non-Medicaid	8.5	−2.5*
p-value for impact difference between subgroups[c]	--	(.11)
Non-Medicare Reimbursed Aide, Homemaker, and Maid Care (Percent)		
Medicaid	15.2	−0.2
Non-Medicaid	23.0	0.7
p-value for impact difference between subgroups[c]	--	(.86)
In Two Weeks Before Interview[d]		
Aide, Homemaker, or Maid Visits (Number)		
Medicaid	0.7	−0.1
Non-Medicaid	0.8	-.001
p-value for impact difference between subgroups[c]	--	(.86)

SOURCE: Four-month patient interview.

NOTE: Observations are weighted to represent agencies by size. The analysis includes only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 471 to 488; for the treatment group, the sample sizes range from 275 to 287; for the control group, the sample sizes range from 196 to 203. For the non-Medicaid group, the overall sample sizes range from 1,509 to 1,558; for the treatment group, the sample sizes range from 725 to 756; and for the control group, the sample sizes range from 784 to 803.

TABLE C.2 *(continued)*

*Significantly different from zero at the .05 level, two-tailed test.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for clustering and weighting, except that no manual adjustment was made for aide, homemaker, or maid visits, because the design effect was trivial.

[d]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of visits, including episodes for which the patient received no aide, homemaker, or maid visits during the two-week period. In testing hypotheses that the subgroup impacts are different from zero, we did not adjust manually for clustering and weighting because the design effect was trivial.

TABLE C.3

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF FORMAL, NONRESIDENTIAL, NON-MEDICARE SERVICES AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]

(Observations Weighted to Represent Agencies, by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Four-Month and Eight-Month Interviews[b]		
Home-Delivered Meals (Percent)		
Medicaid	14.4	2.5
Non-Medicaid	4.5	2.4*
p-value for impact difference between subgroups[c]	--	(.75)
Non-Medicare Reimbursed Aide, Homemaker, and Maid Care (Percent)		
Medicaid	12.0	2.8
Non-Medicaid	21.1	-0.9
p-value for impact difference between subgroups[c]	--	(.40)
In Two Weeks Before Interview[d]		
Aide, Homemaker, or Maid Visits (Number)		
Medicaid	0.8	-0.1**
Non-Medicaid	0.9	-0.1***
p-value for impact difference between subgroups[c]	--	(.93)

SOURCE: Eight-month patient interview.

NOTE: Observations are weighted to represent agencies by size. The analysis includes only episodes reflecting a patient's first admission.

The overall sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 429 to 452; for the treatment group, the sample sizes range from 248 to 262; and for the control group, the sample sizes range from 181 to 192; for the non-Medicaid group, the overall sample sizes range from 1,384 to 1,414; for the treatment group, the sample sizes range from 676 to 690; and for the control group, the sample sizes range from 708 to 727.

TABLE C.3 *(continued)*

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for the effect of clustering and weighting. The adjustment was made manually for aide, homemaker, or maid visits.

[d]These estimates, obtained from Tobit models, represent treatment-control difference in the average number of visits, including episodes for which the patient received no aide, homemaker, or maid visits during the two-week period. In testing hypotheses that the subgroup impacts are different from zero, we adjusted manually for clustering and weighting.

*Significantly different from zero at the .10 level, two-tailed test.
**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE C.4

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL SERVICES AT FOUR MONTHS, BY PATIENT'S MEDICAID STATUS[a]

(Observations Weighted to Represent Agencies, by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Home Health Admission and Four-Month Interview		
Resident in Assisted-Living Facility (Percent)[b]		
Medicaid	8.9	-3.8
Non-Medicaid	10.2	-2.3
p-value for impact difference between subgroups[c]	--	(.53)
Nursing Home Admission (Percent)[b]		
Medicaid	6.0	-5.4**
Non-Medicaid	5.1	-1.2
p-value for impact difference between subgroups[c]	--	(.21)
Nursing Home Days (Number)[c]		
Medicaid	3.0	-3.8***
Non-Medicaid	2.1	-0.4***
p-value for impact difference between subgroups[c]	--	(.36)

SOURCE: Four-month patient interview.

NOTE: The analysis includes only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 493 to 496; for the treatment group, the sample sizes range from 286 to 288; and for the control group, the sample sizes range from 202 to 208. For the non-Medicaid group, the overall sample sizes range from 1,533 to 1,567; for the treatment group, the sample sizes range from 756 to 761; and for the control group, the sample sizes range from 797 to 806.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

TABLE C.4 *(continued)*

[b]These estimates, obtained from logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These *p*-values have been adjusted to account for clustering and weighting.

[d]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including days of patients who were not residents of nursing homes. In testing hypotheses that the subgroup impacts are different from zero, we have not adjusted for clustering and weighting because the design effect was trivial.

**Significantly different from zero at the .05 level, two-tailed test.
***Significantly different from zero at the .01 level, two-tailed test.

TABLE C.5

IMPACT OF PER-EPISODE PAYMENT ON THE USE OF RESIDENTIAL SERVICES AT EIGHT MONTHS, BY PATIENT'S MEDICAID STATUS[a]

(Observations Weighted to Represent Agencies, by Size)

Dependent Variable	Control Group Mean	Impact (*p*-Value)
Between Four-Month and Eight-Month Interviews		
Resident in Assisted-Living Facility (Percent)[b]		
Medicaid	10.4	-4.0
Non-Medicaid	10.1	-2.8
p-value for impact difference between subgroups[b]	--	(.70)
Nursing Home Admission (Percent)[b]		
Medicaid	6.6	-3.1
Non-Medicaid	5.4	-4.2***
p-value for impact difference between subgroups[c]	--	(.39)
Nursing Home Days (Number)[d]		
Medicaid	8.8	-7.9***
Non-Medicaid	7.8	-6.9***
p-value for impact difference between subgroups[c]	--	(.97)

SOURCE: Eight-month patient interview.

NOTE: Observations are weighted to represent agencies by size. The analysis included only episodes reflecting a patient's first demonstration admission.

The sample sizes vary as a result of missing values. For the Medicaid group, the overall sample sizes range from 445 to 457; for the treatment group, the sample sizes range from 257 to 265; and for the control group, the sample sizes range from 188 to 192. For the Non-Medicaid group, the overall sample sizes range from 1,401 to 1,418; for the treatment group, the sample sizes range from 677 to 690; and for the control group, the sample sizes range from 724 to 728.

[a]Medicaid status is measured as whether or not the Medicaid program purchased Medicare coverage for the patient.

TABLE C.5 *(continued)*

[b]These estimates, obtained logistic regression models, represent treatment-control differences in the probability of receipt of the service. In testing hypotheses that the subgroup impacts are different from zero, we adjusted for clustering and weighting.

[c]These estimates, obtained from Tobit models, represent treatment-control differences in the average number of days, including days of patients who were not residents of nursing homes. In testing hypotheses that the subgroup impacts are statistically different from zero, we adjusted for clustering and weighting.

***Significantly different from zero at the .01 level, two-tailed test.

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